SOUTHWESTERN JOURNAL OF ANTHROPOLOGY

Published by the University of New Mexico and the Laboratory of Anthropology, Santa Fé, in the interest of general anthropology

VOLUME 15 • NUMBER 2 • SUMMER • 1959

The Potter's Wheel: an Analysis of Idea and Artifact in Invention — George M. Foster — 99

Theory and History in American Archaeology — Earl H. Swanson, Jr. — 120

The Inca Occupation of the South Coast of Peru — Dorothy Menzel — 125

Magic and Witchcraft: an Interpretation from Dissonance Theory — Theodore Rosenthal and Bernard J. Siegel — 143

A Prehistoric Population Estimate Using Midden Analysis and Two Population Models — Robert Ascher — 168

Some Notes on Acoma Kinship Terminology — Wick R. Miller — 179

Itzamna Cocahuat, the Possible "Speech-Bird" God of the Maya — Floyd Thurber and Valerie Thurber — 185

The Appearance of the Mounted Indian in Northern Mexico and the Southwest, to 1680 — Jack D. Forbes — 189

Leadership and Status Conflict on Rarotonga, Trust — Marc J. Swartz — 213

UNIVERSITY OF NEW MEXICO
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THE SOUTHWESTERN JOURNAL OF ANTHROPOLOGY is designed to include articles in all branches of anthropology relating to peoples and cultures, past and present, in any region. It is offered as a vehicle of expression for anthropologists in all parts of the world.

Manuscripts and other editorial materials should be addressed to the Editor, LESLIE SPIER, University of New Mexico, Albuquerque, New Mexico.

Subscription: $4.00 a calendar year (single number, $1.00). Foreign subscription, 1 £ sterling or its equivalent in local currency. Subscriptions should be sent to W. W. HILL, University of New Mexico, Albuquerque, New Mexico. (Remittance should be made payable to the SOUTHWESTERN JOURNAL OF ANTHROPOLOGY.)

Issued in quarterly numbers: Spring, Summer, Autumn, Winter of each year.

NOTE: THE SOUTHWESTERN JOURNAL OF ANTHROPOLOGY is not available in exchange.

Indexed in the *International Index to Periodicals*: abstracted in *Sociological Abstracts*

Microfilm copies of whole volumes of the SOUTHWESTERN JOURNAL OF ANTHROPOLOGY may be purchased for permanent files by libraries and other subscribers from UNIVERSITY MICROFILMS, 313 N. First Street, Ann Arbor, Michigan, to whom inquiries should be addressed.

Printed at the University of New Mexico Printing Plant.

Entered as second-class matter June 17, 1947, at Albuquerque, New Mexico, under Act of Congress of July 16, 1894.

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OF ANTHROPOLOGY

VOLUME 15 • NUMBER 2 • SUMMER • 1959

THE POTTER'S WHEEL: AN ANALYSIS OF IDEA AND ARTIFACT IN INVENTION*

GEORGE M. FOSTER

WHAT IS AN INVENTION? Is it simply a new mechanical device, a machine, a material object that can be described objectively? Is it an understanding on the part of a user of the full potential, or the developmental possibilities of an apparatus? Is it the discovery of a principle, or the application of a principle to a new purpose? Let us take as illustration the potter's wheel. It may be described in material terms as a pair of horizontal discs joined by an axle, supported in such fashion that it rotates easily and rapidly. Its mode of use may be described equally briefly: the potter places clay on the upper disc, sets the apparatus in motion by kicking the lower disc, and forms the vessel with his hands. Yet both of these statements miss the significant point. The most fundamental characteristic of the true potter's wheel is that it is (or incorporates) a flywheel that provides momentum and continuous rotation so that centrifugal force can be utilized to "throw" a pot from soft plastic clay. The essence of the invention of the potter's wheel, and its cultural consequences, is not elaboration of a material form, but the idea, the recognition of the possibilities of exploiting this centrifugal action.

Although in our technologically-minded society the man in the street tends to think of invention in terms of material forms, anthropologists recognize that the idea and not the artifact is the culturally-significant thing. Kroeber, for

* This is the second of two articles dealing with separate but related problems. The first, *The Coyotepec* Molde *and Some Associated Problems of the Potter's Wheel*, appeared in Southwestern Journal of Anthropology, vol. 15, pp. 53-63, 1959. The point in that article that bears on this discussion is that male and female Zapotec Indian potters in Coyotepec form vessels on unpivoted pottery saucers which, for the finishing of the neck, are revolved as rapidly as 90 revolutions per minute, well within normal working ranges of speed of wheel-using potters.

example, believes that basic inventions are few. "What is new is often a new function, which is culturally significant because it is socially accepted, and which we can name." Speaking of the distinction between material and non-material culture, he writes that "What counts is not the physical ax or coat or wheat but the idea of them, the knowledge how to produce and use them, their place in life."[1] Spier points out that the popular concept of an invention represents a narrowing of its original meaning, and that "it is equally proper to speak of the invention of an idea."[2] Herskovits likewise calls attention to the tendency, even on the part of men such as Dixon and Harrison, to focus on material objects when considering the nature of invention and innovation, thereby diverting attention from the importance of underlying ideas.[3]

Granted that anthropologists recognize the distinction between idea and artifact in invention, do they in fact consistently maintain this distinction in drawing conclusions? In this paper I suggest that in the case of the potter's wheel—idea and artifact—those who have written on the subject frequently do not, and as a consequence commonly accepted theories about the origin of the potter's wheel, as a material item, and its cultural consequences may need significant revision. Specifically I hope to show that:

(a) The apparent failure fully to distinguish between the material form of the potter's wheel and the mechanical principle involved—the use of centrifugal force to "throw" a vessel—has led to unwarranted conclusions about and interpretations of some aspects of culture history.

(b) The common assumption among American anthropologists that the potter's wheel derives from the cart wheel is probably erroneous, and that an evolutionary sequence from movable base to pivoted turntable to true wheel seems much more likely.

When anthropologists write about the potter's "wheel" and its first appearance in history, they appear to have in mind a concrete object, generally not well described, which somehow introduced an entirely new dimension into the pottery-manufacturing process, which in turn led to significant cultural and social changes. This sequence can be summarized as follows: In pre-wheel times women made pots, primarily for their own domestic consumption. Pot-making techniques were laborious and time-consuming, so that absolute production was strictly limited. The wheel greatly shortened the time necessary to make a vessel, reduced the physical effort involved, and simultaneously improved the quality of the ware. Full-time male specialists essentially eliminated women from pottery work, thus

1 Kroeber, 1948, pp. 362, 295.
2 Spier, 1958, p. 93A.
3 Herskovits, 1948, pp. 494-496.

commercializing a home craft. Greatly increased production stimulated the development of trade and markets.

Authors are in remarkable agreement about these points. Chapple and Coon state the case for pre-wheel work: "Fashioning pottery by hand is usually a woman's job, and she makes her pots mostly for use in her own household, although she may sell a few in market."[4] With the appearance of the wheel Brew finds that "A potter could shape a pot much more quickly and also more precisely [than by hand]. . . . The invention of the potter's wheel brought an end to pot-making as a housewife's task in the Near East."[5] The supposed speed of the wheel has attracted much attention. Turney-High believes that "The invention of the potter's wheel enabled the artisan to make from ten to twenty vessels in the time formerly required to produce one, thus introducing mass production in ceramics. Pottery thenceforth became a cheap and easily manufactured commodity and the standard of living consequently rose for the bulk of the people."[6] Childe is even more impressed with the speed of the wheel: ". . . a professional can shape in ten minutes a vessel that might take a housewife ten hours to build by hand."[7] Linton likewise notes the possibilities of increased production: "This contrivance [the wheel] made it possible to mass produce and standardize pottery. . . ."[8] Harrison indicates a similar belief when he writes that "The invention of the potter's wheel brought about an important change in the potter's art, since it enabled him to model his clay with a new speed and certainty, and thus led in the direction of industrialization."[9] And finally, Laufer finds that the modifications brought about in industry by the application of the wheel were far reaching: "Technically they led to a greater rapidity and hence intensity of the process. . . . It was due to the introduction of the wheel that ceramic labor was afforded the opportunity of growing out of a mere communal, clannish, or tribal industry into a national and international factor of economic value."[10]

These assumptions about wheel-made pottery, and the socio-cultural consequences that follow, can be accepted only if the ultimate potential of the potter's wheel—the use of centrifugal force to throw vessels—was comprehended and exploited by early wheel-using potters. I believe, however, and will attempt to show, that this probably was not the case. Among many modern "wheel"-using potters, the principle of throwing is but scantily exploited, and it seems likely that this was also true five thousand years ago. The alleged advantages of the wheel,

4 Chapple and Coon, 1942, p. 124.
5 Brew, 1956, p. 126.
6 Turney-High, 1949, p. 174.
7 Childe, 1954, p. 204.
8 Linton, 1955, p. 114.
9 Harrison, 1928, p. 36.
10 Laufer, 1917, pp. 161-162.

then, in fact may not have existed, and the changes that appear to result from its introduction must have been due, in part at least, to other causes.

It is striking to note how casually the nature of the wheel and its mode of use are dealt with by most anthropologists. Usually it is taken for granted that a potter's wheel is a potter's wheel, that it is used in much the same fashion wherever it is found, and that this fashion stands in contra-distinction to all other pottery-making techniques. Herskovits, for example, writes that "the two broadest categories of pottery-making are those which distinguish pottery made by hand and by the use of the potter's wheel."[11] Beals and Hoijer contrast hand-building—coiling, modeling, and molding—with the use of a "turntable or potter's wheel."[12] Spier, dealing with historical problems, distinguishes two levels of pottery development in time and area of occurrence: vessels shaped by hand alone, and "pottery spun on a wheel."[13]

Perhaps some of the confusion stems from space limitations in introductory texts, where only brief statements can be devoted to many complicated subjects. Nevertheless, although these statements are correct as far as they go, they fail to make clear an absolutely basic point: pottery-making is most accurately thought of as a continuum from hand modeling to full exploitation of the wheel, with innumerable intermediate gradations. The dividing line between the poles, or two major divisions, can be drawn on the basis of one or the other of two criteria, but not on the basis of both. Usually the line is drawn on the basis of the presence or absence of a wheel or wheel-like device—in short, on the basis of material form, or artifact. If we grant, however, that in invention the idea is the significant thing, the line should be drawn on the basis of the mechanical principle utilized; in this case, the presence or absence of centrifugal force utilized to throw a vessel. The two lines may on occasion coincide, but this is coincidence, and irrelevant to the present argument.

Several anthropologists describe the use of the wheel, in its early or modern form, for introductory students. Titiev says that "A lump of prepared clay would be thrown on the center of a revolving disc, and as it spun the potter manipulated it until it took the shape he had in mind."[14] Hoebel writes that "In the use of the wheel, the lump of clay is centered directly over the axle, and in the hands of a skillful worker the revolving mass seems miraculously to grow up and out into a vase."[15] Howells notes that, historically, "The wheel was also promptly employed to speed up pottery making, by turning it as it was shaped. . . ."[16] Beals and

11 Herskovits, 1948, p. 263.
12 Beals and Hoijer, 1956, pp. 264-265.
13 Spier, 1956, p. 235.
14 Titiev, 1954, p. 245.
15 Hoebel, 1949, p. 143.
16 Howells, 1954, p. 318.

Hoijer tell the student that "The most efficient method of shaping pottery is by rotating a lump of clay on a turntable or potter's wheel. With the hand or an implement, the rotating clay is quickly shaped into the desired form."[17] Linton, in a surprisingly naive statement, says that "The potter's wheel, in its simplest and most universal form, is really an ancient style pair of disk cart wheels with axle attached. The axle is set vertically and the operator spins the lower wheel with his foot while with his hands he spreads and shapes the soft clay upon the upper wheel."[18]

These are pretty sketchy descriptions if the idea is to acquaint the reader with what a pottery's wheel is and can be made to do. Except for Titiev, the principle of centrifugal force making possible "throwing" is, if not ignored, at least not made specific. The other authors use such expressions as "turning," "rotating," and "spinning."

These words are of limited value in explaining the potential superiority of the wheel over other techniques. As Scott points out, "whether a pot is modelled from a lump of clay or built up, it is necessary to rotate the vessel as it is shaped."[19] Although occasionally a pot remains stationary and the potter moves around it while working, in most cases a description of even the simplest pottery-making techniques requires the use of words such as "turning" and "rotating," so that they are not particularly distinctive when used to describe the function of the wheel.

Among anthropologists, Childe is almost unique in his generally accurate description of the principle of the wheel: "In pottery manufacture the function of the wheel is to supply centrifugal force to a lump of still plastic clay accurately 'thrown' on its centre. Such a well centred lump when spinning fast—100 revolutions per minute at least are required—needs only light guiding-pressure from the potter's hand to rise and assume any sectionally circular form he may wish to impose upon it. Instead of expending his own muscular energy in pressing, moulding, or coiling the clay, he merely directs energy imparted to it."[20] Childe is wrong on two points. As pointed out in the first article of this series, average wheel speeds often fall well below the 100 revolutions per minute range.[21] Furthermore, the process of accurately "centering" clay on the wheel requires great physical effort; female potters in Western society often find this extremely fatiguing.

Scott likewise makes clear the principle of a rotating disc as applied to pottery: the shaping of pots by "throwing," that is, "by spinning on a rapidly rotating wheel, the hands guiding the shaping instead of directly effecting it."[22] And again, "The

17 Beals and Hoijer, 1956, p. 265.
18 Linton, 1955, p. 114.
19 Scott, 1954, p. 387.
20 Childe, 1954, pp. 195-196.
21 Foster, 1959, p. 62.
22 Scott, 1954, p. 378.

potter's wheel . . . introduced the entirely novel principle of 'throwing' pottery, i.e. of shaping the plastic clay by spinning, the energy being derived from the rapidly rotating wheel-head and not from the potter's fingers."[23] And finally, Shepard points out, in discussing the possible applicability of wheel concepts to certain New World native techniques, "The critical question in regard to rotation of the vessel during construction is whether or not sufficient speed is attained for rotary and centrifugal forces to play a part in the shaping process."[24]

In my opinion the potter's wheel, and its role in civilization, can best be understood if it is examined—to paraphrase Childe—in the sense of the principle of the "utilization of the centrifugal force imparted to a fast-spinning lump of clay, rather than any particular device for making the lump spin."[25] Centrifugal force can be so used with no wheel at all, as in the case of Coyotepec, and the wheel can be used as an aid in pot-making—and often is—without necessarily using the principle of centrifugal force. The potter's wheel, as a tool, did not bring about the technical and social advances and changes commonly attributed to it; the wheel merely paved the way to these changes. In some parts of the world the wheel's potential has been fully exploited, and in other places it has been but partially exploited. The tool may be the same in both cases, but the consequences are very different.

At this point it will be profitable to examine the several major historical forms of the potter's wheel, particularly with respect to their capacity for "throwing."

I. *The "double" or "kick" wheel.* Contrary to Linton[26] this is not the "simplest and most universal form;" it is the most complex and, in many respects, the least universal form. But since it is a common wheel found among artistic or home potters in the United States, and is perhaps the only form of wheel (along with the more specialized treadle wheel) observed by many anthropologists, the potential of this perfected device is assumed for all wheels, whatever their form may be. This wheel is characterized by two horizontal discs rigidly joined by a vertical axle, supported at the bottom by a thrust bearing (in the case of crude wheels, a depression in a stone) and beneath the head by a second bearing. The potter, usually sitting but sometimes standing, imparts motion to and controls the speed of the instrument by kicking the lower wheel which, in addition to serving as the source of power, functions also as a fly wheel to govern the speed and give momentum. In the average American wheel it weighs somewhat more than 100 pounds. Potter's clay is centered for working on the top wheel (or "head"), which often

23 *Idem*, p. 388. Scott also overlooks the effort involved in "centering" clay on the wheel.
24 Shepard, 1956, p. 60.
25 Childe, 1954, p. 203.
26 Linton, 1955, p. 114.

but not always is smaller than the bottom wheel. The double wheel revolves smoothly, without oscillation, and normally is associated with true "throwing." Since thrown ware requires a paste more moist than most hand-made ware, the the resulting object may sag during drying, unless it is made heavier and thicker at the foot than desired. When leather-hard this heavy and thick object (*ébauche* in French—I know of no corresponding term in English) undergoes a lathe-like process on the wheel whereby, with cutting instruments, it is trimmed to its final form. This process, known as "turning" or "trimming" (*tournassage* in French), is a basic requirement for most thrown ware, and it would appear to require a wheel that oscillates little if at all.

The kick wheel, in its common form, is in use from the Indus basin west across Asia, in north Africa, and in pre-industrial and peasant Europe. Variant forms in China, Japan, and Java are described by Laufer.[27] The presence of the kick wheel outside those areas is due to diffusion since the Age of Discovery. Indications as to the probable date of appearance of this wheel are notably scarce in the literature. Childe argues (unconvincingly to me) for a time as far back as 3,000 BC, i.e., shortly after the appearance of the simplest potter's wheel (3,250 B.C. ± 250 years), again to use Childe's dating.[28] Frankfort believes it to be "a relatively modern invention," i.e., in the time of the Ptolemys,[29] a date concurred in by Harrison as well.[30] Simple logic, whatever its shortcomings in historical reconstruction, suggests that the kick wheel must be the last stage in a long evolutionary line.

II. *The "simple" wheel.* The simple wheel differs from the double wheel in that it takes more forms, and has distinct characteristics. It resembles the double wheel in that it can be used to throw a pot to produce *ébauche* ware, and in its non-oscillating forms it can be used for turning. The simple wheel, as the term implies, consists of a head, usually of such weight and proportions as to allow sufficient momentum to permit throwing of a pot after an initial application of force, usually by means of a stick. The famous Breton potter's wheel is perhaps the most developed of these forms.[31] The enormously heavy Indian and East Pakistan spoked wheel is another,[32] as is the large solid Indian wheel.[33]

Childe points out a significant difference, frequently not made explicit in descriptions, in the way this wheel is mounted: in some forms there is a socket in

27 Laufer, 1917, pp. 164-168.
28 Childe, 1954, p. 199.
29 Frankfort, 1924, p. 8.
30 Harrison, 1928, p. 41.
31 Pictured, among other places, in Scott, 1954, p. 389 and Franchet, 1911, p. 58.
32 E.g., Foster, 1956a; Gait, 1897; Holder, 1897. I estimate the weight of such wheels I have examined at 100-150 pounds.
33 E.g., Dobbs, 1897.

the underside of the wheel into which a fixed non-revolving pivot projects; in the other form, the pivot is fixed in the underside of the wheel and revolves with it, its lower end fitting into a fixed socket on the ground.[34] The "socketed" variant, to use Childe's terminology, is found in much of India,[35] and east into China and Japan.[36] In all India this socketed variant, because of slipping in the shallow hole into which the fixed pivot projects, oscillates as it spins. It permits throwing, but would appear to preclude turning (i.e., *tournassage*), or at least render it very difficult.

If the socket is sufficiently deep, however, as illustrated by Childe for the Japanese wheel,[37] oscillation should be reduced to a minimum and perhaps eliminated entirely. Chinese wheels pictured by Franchet, activated by an assistant, either by toe or by a pulley, appear to be non-oscillating, and thus to permit turning.[38]

The pivoted variant is illustrated by Childe for Madras.[39] With a short pivot, this wheel is subject to the same problem of oscillation as the socketed variant. By increasing the length of the pivot, and by steadying it with a perforated horizontal cross bar, oscillation can be eliminated. This appears to be the case with the modern Cretan simple wheel, described and illustrated by Xanthoudídes.[40]

An interesting evolutionary point is illustrated by the two variants: in the primitive form, it would appear that neither has an advantage over the other. Both are set spinning in the same way, both oscillate, and both can be used to produce essentially the same kind of ware. With technical improvement, the oscillation can be eliminated from both. But in the case of the socket variant this improvement, a deep socket, is the end of the road. The wheel is now as perfect as it ever can be, and in terms of the potter's art it proves to be an evolutionary blind alley. Extension of the revolving pivot, however, eventually leads to an axle, to which the foot wheel is fastened, thus giving rise to the double wheel. If the Cretan simple wheel (which exists side by side with the double wheel) is in fact a survival from very ancient times, it gives a clue as to why the kick wheel was certainly invented in the West, and not in the East.

III. *The pivoted turntable.* This consists of a disc, usually of wood, with a hollow or socket on the underside that is balanced on a fixed boss or pivot. This

34 Childe, 1954, pp. 197-198.
35 Foster, 1956a; Dobbs, 1897.
36 Childe, 1954, p. 197.
37 *Ibid.*
38 Franchet, 1911, pp. 62-63.
39 Childe, 1954, p. 198. This drawing is taken from Holder, 1897.
40 Xanthoudídes, 1927.

type has been described and/or illustrated for the Gelib of Italian Somaliland by Childe,[41] in the Philippines by Foster,[42] in the Malay Peninsula by Wray,[43] in the Congo by Franchet,[44] and in many other places. The pivoted turntable, in the forms commonly described, does not have sufficient weight, and does not run on a sufficiently smooth bearing, to furnish the centrifugal force that is essential to throwing. Hence, being in no sense a fly-wheel, the turntable is not truly a potter's wheel.

The mechanics of the pivoted turntable (and perhaps even the simple wheel) have been misunderstood by many writers, including such authorities as Frankfort and Franchet. The former speaks of a pivoted slow-turning wheel kept moving by the potter "who in that case only has one hand continuously free for the shaping,"[45] while the latter, for all his technical knowledge, contrasts the wheel which "laisse donc au potier la liberté des ses deux mains" with the simpler pivoted forms which "ne lui laisse la liberté que d'une seule: pendant que la main droite ébauche le vase, la gauche imprime le mouvement à la girelle."[46] On any rotating device in which centrifugal force plays a part in the shaping of a vessel, both hands are essential, one on the inside and one on the outside of the pot. Laufer quite correctly questions reports in which a wheel or wheel-like device is turned with the left hand, leaving only the right hand free,[47] while Harrison quite bluntly—and equally correctly—says that such reports are contrary to the facts of observation.[48] Where I have seen the turntable used, both hands shape the vessel, turning it slowly. The turntable revolves, not because force is applied directly to it, but because force is applied to the clay which it supports.

There is only one stage in the forming of a vessel in which centrifugal force, the basis of throwing, can be exploited by one hand and this, as I hope to show later, may be critical in the definition of the wheel. Many potters who build vessels in a variety of ways finish the neck by rapidly rotating the pot on some type of wheel, the thumb of the right hand opposed to the fingers, on opposite sides of the clay wall. For this operation the left hand, if needed, is free to impart continuous motion to the rotating object. Since, in such operations, the speed of rotation is rapid, and since pressure simultaneously is exerted inside and outside the pot, and since centrifugal force supplies the power to shape, I believe it is correct to describe such rim and neck work as "throwing."

IV. *The unpivoted turntable.* Were it not for the case of Coyotepec, and the

41 Childe, 1954, p. 196.
42 Foster, 1956b.
43 Wray, 1903.
44 Franchet, 1911, p. 54.
45 Frankfort, 1924, p. 8.
46 Franchet, 1917, p. 163.
47 Laufer, 1917, p. 163.
48 Harrison, 1928, pp. 39-40.

"thrown" rims of the ware made there, this fourth category would have to be omitted from a consideration of wheel-pottery. But even a single case of an unpivoted turntable that is revolved at speeds far higher than the usual pivoted turntable, speeds equal to those of the double wheel, must be taken into account in considering the nature of the wheel. The fact that much hand-made pottery, whether coiled or modeled from a single lump, or otherwise built, is placed in a clay saucer or on a wooden disc that turns, is well known, and reported from far-distant points in the Old World and the New. The Mexican variants have been described in the first article of this series. The kabal, as was pointed out, has even been referred to as a primitive wheel. The kabal, turned by feet or toes rather than hands, often is thought to be a development unique to Yucatan. But in addition to the geographically marginal kabal of Acatlán, Puebla, toe-turning of unpivoted supports is reported for East Pakistan,[49] and for the Kabyle of Algeria.[50] Foot-turning of the simple pivoted turntable is also reported for the Gelib of Italian Somaliland by Childe.[51] I suspect that in the cases cited rotation is so slow as to preclude the expression "throwing," even for the neck and rim.

Because of the confusion that surrounds the term, I have until now avoided the French word *tournette*. Franchet uses the term to describe forms ranging in complexity from the simple pivoted turntable of the Lower Congo[52] to the early Egyptian and Greek simple wheels depicted on vases[53] to the hand-activated spoked wheel of Brittany.[54] In one place he calls the spoked Hindu wheel a *tour*[55] while in another place he refers to it as a tournette.[56] The modern Cretan simple wheel is described by Franchet as a tournette,[57] a usage followed by Xanthoudides.[58] Montandon appears to draw his terminology from Franchet: the simple, pivoted turntable is called tournette,[59] and so is the immensely more complex Breton wheel.[60] In short, Montandon lumps the simple wheel with the turntable, and calls both by the same term. Frankfort describes the tournette as a pivoted "slowly turning wheel,"[61] but he suggests that the much more complex Chinese wheels, turned by an assistant, are also tournettes.[62] The "true potter's wheel," according to Frankfort's scheme, follows directly upon the tournette (in a classificatory sense, and not necessarily an evolutionary line). Harrison says of the Lower Congo simple pivoted turntable that "its method of use makes it

49 Foster, 1956a, p. 402.
50 Van Gennep, 1911, p. 292.
51 Childe, 1954, p. 196.
52 Franchet, 1911, p. 54.
53 *Idem*, p. 56.
54 *Idem*, p. 57.
55 *Idem*, p. 61.
56 Franchet, 1917, p. 44.
57 *Idem*, p. 57.
58 Xanthoudides, 1927, p. 121, 123.
59 Montandon, 1934, p. 518.
60 *Idem*, p. 519.
61 Frankfort, 1924, p. 8.
62 *Ibid*.

difficult to deny it the title of a potter's wheel"[63] yet on the following page he applies the term tournette to the heavy, momentum-sustaining simple wheels of India and other places.[64] MacKay calls a disc of stone or wood supported on a pivot, turned by the potter or an assistant, a tournette, distinguishing it from the Indian cart-wheel form which he calls a "spun" or "quick" wheel.[65]

In view of this confusion one is not surprised to read in Childe that "English archaeological literature has been bedevilled by the translation of the French tournette by the self-contradictory term 'slow-wheel'."[66] He restricts the term to turntables "on which the pot is built, not thrown," a definition I would accept, provided it is amended to include only the pivoted turntable. I would therefore exclude the Mayan kabal and its unpivoted relatives in all parts of the world, as well as the Coyotepec molde.

To recapitulate, and this time in reverse order, it seems to me that four basic types of implements from simple to complex, can be delineated in which rotary motion associated with centrifugal force in some degree is used. These are:

I. The unpivoted turntable, be it the kabal, the Coyotepec molde, the Southwestern puki, the Royerbazar (East Bengal) shanch,[67] or any of the other similar devices known to primitive potters. Customarily pots are modeled or built on these supports, which turn very slowly.

II. The pivoted turntable. To this, and this alone, I would apply the term tournette. It should be noted that the best tournettes, and the best unpivoted turntable (the Coyotepec molde) both permit the use of centrifugal force to throw the rim of vessels. But with this reservation: throwing is not characteristic of the manner of use of these instruments.

Both the unpivoted turntable and the tournette, as here defined, may be utilized by members of either or both sexes, depending on the local cultural tradition.

III. The simple wheel, whether socketed or pivoted. These instruments normally are used to throw part or all of a pot. Those that do not oscillate may be used for turning *(tournassage);* those that oscillate apparently cannot be so used, or at least only with great difficulty. My impression is (although not backed by extensive research) that, excluding China and Japan, more often than not the simple wheel is used only in one of several forming operations in the manufacture of a pot. In short, its potential for throwing is not fully realized; at least, it is not fully exploited.

IV. The double or kick wheel. This, the most advanced of wheels, might

63 Harrison, 1928, p. 38.
64 *Idem*, p. 39.
65 MacKay, 1930, p. 128.
66 Childe, 1954, pp. 196-197.
67 Foster, 1956a, p. 402.

logically be expected to be used only for throwing and *tournassage*. Yet, as I will point out, there are many potters who, while using it, are unaware of its real potential; for them it is more properly a turntable.

In non-industrial societies the simple and compound wheel normally are used only by men.

I now return to my original hypothesis, namely that the cultural significance of the wheel is a function, not of the overt form of the machine itself, but of the potter's ability fully to exploit it by throwing ware. I propose to do this by showing that, in countless places where an instrument capable of throwing a pot is known, its potentialities in this direction are but partially used, if at all. Frankfort is one of very few authorities who points out the fact that much ware can neither be classified as wheel-made nor hand-made, since various combinations of the two methods are used.[68]

One of the most widespread of such dual methods is common to much of India. The heavy simple wheel, which usually oscillates, is used to throw a bottomless cylindrical form, roughly resembling a pot, but with very thick walls. After partial drying, this cylinder is enlarged and beaten into form with paddle and anvil.[69] Although the pot is, in its initial stage, thrown, it is doubtful that any archaeological evidence could give a clue to this (except for the rim), so completely is the wheel evidence obliterated by the paddle and anvil. The heavy simple wheel is used in an entirely different combination process in Maheswar Pasha, East Pakistan. Here the rim and upper half of a pot is wheel-thrown by a man, while the bottom half is molded over an unfired clay convex form by a woman. Then the two halves are welded together with paddle and anvil by a male potter, but the vessel is not further enlarged, as in the more common Indian pattern.[70] In Royerbazar, also in East Pakistan, a male potter throws a rim and shoulder on a simple wheel. After it has dried a couple of days a female potter places the rim upright over a depression in the ground which serves as a mold, passes a thin clay pancake through the opening, and with a pottery anvil molds it to the shape of the depression and welds its edges to the shoulder of the wheel-thrown piece. After more drying the vessel is placed in the shallow depression of a wooden mold and further pounded from the inside.[71] Again in Royerbazar, a male potter may throw the rim and shoulder as just described. Then he pounds a thick pancake into

68 Frankfort, 1924, p. 8.
69 Dobbs, 1897; Dumont, 1952; Foster, 1956a; Gait, 1897; Hallifax, 1894; Holder, 1897.
70 Foster, 1956a, pp. 397-399.
71 *Idem*, pp. 400-401.

concave form in a heavy pottery bowl that serves both as mold and movable support, and with paddle and anvil welds it to the rim.[72]

The double or kick wheel is combined with hand techniques in many parts of the world. MacKay describes how, in a Moslem village near Mohenjo-Daro, the base of a large jar is mold-made, apparently inside a concave saucer. This base, still in its mold, is placed on the wheel, and additional clay is added to build up the sides. Then the rim and shoulder of the pot, previously wheel-thrown, are fitted to the bottom while the original mold is still in place.[73] In this village small vessels are made entirely on the wheel.

In Asyūt Province, Egypt, Blackman describes how a male potter places a thick clay disc on the head of a double wheel, and then builds up the sides to mid-height with thick coils. At this point the vessel is cord-wrapped to prevent sagging, and allowed to dry before it is replaced on the wheel, to have the top half and rim added, again with successive coils. Here the device is no more than a turntable. There is no evidence of throwing, or even spinning in this operation, and photographs reveal the vessel to be crude and asymmetrical, even though made on the most developed form of wheel![74] In another village in the same province a three-man and one woman team use several techniques to produce a pot: the first man prepares and kneads clay; then the woman pounds out the base in a depression in the earth, using a pottery anvil; this thick-walled, shallow bowl is then beaten into larger shape by the second man, who uses his left hand as anvil, and holds a wooden paddle in his right; the last man places the object on a kick wheel, adds a coil of paste, and turns the wheel to form the rim.[75] Franchet describes a somewhat similar operation in the Fayum: paste is molded to form the thick-walled bottom, which is then beaten with a paddle. The neck, wheel-thrown, is then welded on. The technique, says the author, is identical, to judge by form, to that of the Bronze II period in Crete.[76]

In Mexico the kick wheel is used in at least two combinations. In several pottery-making towns in the states of Colima and Jalisco the bottom half of a pot is made in a centuries-old manner, molded over a convex pottery mold. These bottoms are then removed from their molds, placed upright on a kick wheel, and finished by having daubs of clay added to build the vessel to the desired height.[77]

Even more striking evidence of the use of the kick wheel without comprehension of its true potential comes from the village of Ixtaltepec, near the town of Tehuantepec, on the Isthmus of the same name. Here the male potter whom I

72 *Idem*, p. 402.
73 MacKay, 1930, p. 129.
74 Blackman, 1927, pp. 137-139.
75 *Idem*, pp. 140-142.
76 Franchet, 1917, pp. 126-128.
77 Foster, 1948, pp. 368-369.

watched on July 29, 1958 made pots in essentially the same form as Coyotepec potters, substituting his kick wheel for the molde. First he made a crude cone of paste which he punched into the rough form of an "unblocked felt hat," all the while standing and holding the clay in his hands. This object was placed on a flat wooden disc, a bat (which he called molde) stuck to the head of the wheel with a thin film of clay. Then he added daubs of clay, built up the sides, pushed out the walls with a tree-gourd *(jícara)* in the same fashion that a Coyotepec potter uses a *media-luna* pottery instrument of similar form. The potter turned the vessel both ways, at random, by kicking the bottom wheel with one bare foot, while standing on the other while he worked. The speed at this stage was faster than corresponding Coyotepec speeds, but slower than wheel speeds at the same stage. Until the final smoothing the pot remained lopsided. When the vessel had taken its approximate final size and form the potter speeded up the wheel so that by holding his hands motionless he was able to smooth the pot and make it symmetrical. Most of this spinning was clockwise, but even here the potter stopped from time to time, worked at rough spots, and occasionally reversed directions. In this final stage velocity of rotation certainly was within the range of ordinary wheel work, but I was unable to time it precisely. The finished vessel was then left on its bat until it dried.

An equally revealing illustration comes from Spain. In the village of Pereruela, Zamora, female potters coil pottery on a heavy wooden disc balanced on a fixed wooden pivot, and capable of rapid rotation without oscillation because it is joined to a perforated wooden cross supported six inches below the disc, through which the pivot passes. But, although this device, when weighted with clay, has sufficient weight to qualify as a fly-wheel (and hence as a true potter's wheel), the potter turns it slowly as she builds, utilizing it only as a movable platform.[78]

In a second village in Zamora, Moveros, female potters utilize an identical apparatus. Here, however, the potter places a lump of clay of sufficient size for the complete pot on the disc, draws it toward her with several movements of the left hand until it is rotating rapidly, and then models the revolving mass with both hands, repeating the operation as often as needed.[79] Cortés, who describes the operation in both villages, make the point clearly: "The pottery of Moveros, although utilizing a wheel identical to that of Pereruela, is radically distinct in its formation, since in Pereruela the coil method is used, while in Moveros the customary technique of potters who use the kick wheel is used, and coiling is unknown."[80]

78 Cortés, 1954.
79 Cortés, 1958.
80 *Idem*, p. 100.

For purposes of cultural interpretation, the wheel is unknown in Pereruela and known in Moveros, even though the identical artifact is found in both villages. The wheel will not be known in Pereruela until the existing turntable is utilized to throw vessels. An important limitation in archaeological method is here apparent: without knowing *how* an artifact is used, gross error in interpretation is possible. In all probability if a Near Eastern Pereruela of 3,000 BC were excavated and the turntable had survived, the excavator would have no hesitation in describing pottery of the village as wheel-made.

It should be apparent from the examples cited from India, Pakistan, Egypt, Mexico, and Spain that a good many people who are described as "wheel"-using artisans are such only by courtesy. They only partially understand, or do not understand at all, the real potential of their instrument. For many of them—this is particularly true of the Ixtaltepec and Pereruela potters—the wheel is merely an improved mobile platform or simple turntable. Further, it should be clear that the many operations involved in making this "wheel" pottery preclude the speedup, and the vastly increased production, that is the presumed historical consequence of the invention of the potter's wheel.

Actually, it seems highly likely that the first simple potter's wheel—I speak of the artifact, obviously—was used for building, or partially throwing pots, for a long time before the advent of true wheel-thrown ware. Moreover, to judge by the relative slowness of the "wheel"-using potters described in India, Pakistan, Egypt, Spain, and Mexico, a notable speedup in production with consequent market implications cannot be taken for granted. Wheel-throwing *can* increase speed and ease of production, but partial wheel-using probably does not. A visit to any of the great pottery markets of central Mexico—Toluca, Pátzcuaro, Oaxaca, or Mexico City itself—is most instructive. In few if any places in the world is pottery made on such a commercial scale, and transported so far from place of manufacture to place of sale. Yet the overwhelming bulk of the produce offered is made with pre-wheel techniques, and men play as important a role as women in the manufacture. I am inclined to believe that the commercialization of pottery-making is a function of a growing population and trade specialization, and that the presence or absence of the wheel in this process is purely fortuitous.[81] More likely it is the other way around: commercialization may be the motivation that leads potters to abandon traditional techniques, and to undergo the difficult experience of learning new motor patterns, in order to keep up with market demands. Yet

81 Childe is thinking the same thing when he says "A precondition for the use of the wheel is a social surplus sufficient to support the potter and his family—that is, an effective market for his wares" (1954, p. 213).

even this motivation is not necessarily compelling, as evidenced by the resistance of the Mexican potter to the wheel, or its full exploitation, in spite of familiarity with its existence and capabilities.[82] The fact is that competent Mexican potters of both sexes, using hand techniques alone (principally the pottery mold) can make vessels more rapidly than all but the best wheel-throwing potters. The simple Indian wheel, in its usual form, strikes me as a technique far inferior to Mexican mold techniques, whether the criteria of comparison be speed, strength of the vessel, or artistic quality.

Now I wish to reëxamine theories about the origin of the potter's wheel, in the light of the evidence given in this paper. Two principal hypotheses have been advanced:

(a) Transfer of principle from the cart wheel;

(b) An evolutionary sequence roughly similar to the typology described in this paper.[83]

Laufer is a principal exponent of the transfer-of-principle school. He believed, erroneously, that only women make pots by hand. Since the wheel is associated with men, whether on carts or to make pots, all efforts to derive the wheel from simpler techniques are futile. He cites the Indian spoked wheel as evidence of similarity to cart wheels, and emphasizes that in the areas he knew both cart and potter's wheel are called by the same term. The potter's wheel therefore, he recognizes, presupposes an earlier cart wheel, a historical assumption he believed to be fact.[84]

Harrison also argues, in similar vein, although less dogmatically, suggesting that pivoted and unpivoted turntables are degenerate forms of the wheel, rather than possible evolutionary steps. Like Laufer, he cites the spoked wheels of India, but recognizes that the first cart wheels were in fact solid.[85]

To the extent that American anthropologists concern themselves with the problem, the cart wheel hypothesis usually is assumed; this is perhaps the consequence of Laufer's authority, as recognized by an earlier generation. Spier, for example, says the potter's wheel was "by origin a cart wheel."[86] Herskovits feels

82 The very success Mexican potters have had with mold techniques, and the rapidity with which they can produce an excellent quality ware, may explain their resistance to the wheel. The wheel is not enough better, as a technique, to motivate them to give up traditional ways. On the other hand, to potters entering a market economy, knowing only hand techniques inferior to molds, the attraction of the wheel might be sufficiently great as to lead to its use.

83 Coon believes the potter's wheel may have been suggested by the bow drill, but he gives no details (1954, p. 212).

84 Laufer, 1917, pp. 151-159.

85 Harrison, 1928, pp. 37-39.

86 Spier, 1956, p. 235.

that the discovery of the wheel, in the Neolithic, "seems to have been made in connection with transport,"[87] and that subsequently it was applied also to pottery-making. Brew suggests that the wheel was associated with "freighting and charioteering" before it was "discovered that it could be adapted to pottery making."[88] Linton states that "the [potter's] wheel began as a transportation appliance. . . ."[89] And Turney-High confidently asserts that "The fast-spinning potter's wheel awaited the wagon wheel."[90]

The evolutionary sequence hypothesis has been more favored by Europeans, and particularly the French. *All* primitive hand potters, says Franchet, must use something, be it a flat stone or a thick board, on which to rest and turn the vessel they are shaping. One day some primitive potter realized that it would be easier to make the support rotate, rather than the pot, and the [pivoted] turntable came into being. If the rotating pivot were elongated to form an axle, a kick wheel could be attached, and thus we would have the modern double wheel. The simple pivoted wheel (which he here calls tournette) is thus the ancestor of the wheel for Franchet.[91] Montandon fills in the obvious gaps in this sequence, hypothesizing four stages: immovable base, unpivoted turntable, pivoted turntable, and double wheel.[92]

Wu also takes a similar position: "From the evidence that I have collected, I should say that the wheel is derived from the turn-table. The turn-table is a wooden disk pivoted on a board fixed in the ground. Only because its size and weight are insufficient, it cannot be used for throwing. If the disk is increased in size and weight, it is possible to work up a momentum sufficient for the potter to apply pressure to the clay without stopping motion."[93] Peake suggests the origin of the wheel in a mobile stone base to which a pivot was added.[94] Childe likewise speculates that the wheel might have developed out of a pivoted turntable, but reminds us that there is no actual evidence for the priority of this form.[95] Scott seems to imply the possibility of a similar development, but he is not explicit.[96]

I suggest that the evidence from Coyotepec strengthens the hypothesis (which seems to me entirely logical for other reasons as well) that the potter's wheel is the end product of an evolutionary sequence from unpivoted to pivoted turntable, to simple to double wheel. If a Coyotepec potter, with a primitive molde, can achieve speeds up to 90 revolutions per minute, thereby utilizing the principle of centrifugal force against an unmoving hand to shape a rim, the germ of the idea of

87 Herskovits, 1948, p. 264.
88 Brew, 1956, p. 126.
89 Linton, 1955, p. 86.
90 Turney-High, 1949, p. 174.
91 Franchet, 1911, pp. 55-56.
92 Montandon, 1934, p. 518.
93 Wu, 1938, p. 135.
94 Peake, 1933, p. 160.
95 Childe, 1954, p. 196.
96 Scott, 1954, p. 388.

throwing is present. In Coyotepec the pivot has not been developed (or borrowed), but in Ixtaltepec some bright potter (about sixty years ago, according to the guess of modern potters) saw a potter's wheel, and recognized in it an improved molde, and this is what he thinks he has, to this day. Like his Coyotepec fellows, and doubtless like his grandfather, he throws the rim; otherwise his vessels are hand-made.

Perhaps the most common argument against the evolutionary sequence hypothesis is that, since women do not use the wheel, they could not have invented it. How could a turntable customarily used by women, argues van Gennep, develop into a wheel used by men?[97] This type of argument is based on the early assumption, now known to be only partially true in primitive societies, that hand-made pottery is alone the work of women. In many places, we now know, men using hand techniques are equally important in pottery manufacture; this is particularly true in much of village Mexico. It is undeniably true that men have been associated with wheel-made pottery in the Old World since the time of the first pictures showing the process, but this does not preclude the possibility of a period of several hundred years in which a turntable activated by men or women improved to the point of becoming a simple wheel.

Van Gennep argues, succinctly, that the wheel could have been invented only where men were potters, and he suggests that votive or religious pottery may in fact have been made by man, and hence afford the clue.[98] But a situation such as Mexico, and particularly that of Coyotepec, affords a much more likely explanation. Here there is mass production for the market; men as well as women are skilled potters; and the principle of rotation, at speeds from 60 to 90 revolutions per minute, commonly are utilized to throw the rim of pots. It did not happen in the Mexican Coyotepec, but it might have happened in some Old World Coyotepec: a fast-spinning molde somehow acquired a pivot, and eventually it became a simple wheel.

The correlation between male potters and the wheel admittedly is one of the mysteries of history. Herskovits reflects a common interpretation when he describes this association as "another of those irrationalities in culture that so often confront the student," based, he believes, on the principle of adhesion, the more or less arbitrary tie between two apparently unrelated aspects of culture.[99] It seems likely, however, that there is a better explanation, or explanations. Perhaps physical strength is a factor. In the case of India, for example, it requires far more strength and continuous energy to operate the great single wheels than to build pots by

97 Van Gennep, 1911, p. 297.

98 *Ibid.*

99 Herskovits, 1948, p. 264.

hand. To put in motion a crudely pivoted wheel weighing up to 150 pounds, by means of engaging a stick in a notch in the rim, is backbreaking work. And, if centering clay on a finely-balanced ball-bearing American kick wheel is hard work for women, would not the operation be even more so on a primitive wheel with light flywheel and wooden or stone bearings? Studies of the energy involved in various forms of pottery-making may tell us a good deal about the apparent mystery of male potters and the wheel; cultural vagary is hardly the answer.

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THEORY AND HISTORY IN AMERICAN ARCHAEOLOGY

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THE PUBLICATION, in 1958, of *Method and Theory in American Archaeology*,[1] reflects a growing concern, if not preoccupation, with taxonomy. It is not that many such papers have been published. It is that the taxonomic approach to theory is virtually the only one yet made by Americanists. *Sensu strictu*, the book might better have been titled, "Classification and Interpretation in American Archaeology."

The Willey and Phillips volume is an expression of a "universal" taxonomy, and says little indeed about the kinds of problems that are the subject of historical or scientific theory. The authors have, in fact, taken steps to establish a five-fold developmental classification of technological stages in American prehistory, by means of which New World archaeology is to be given a previously unknown coherence. Furthermore, the authors— and perhaps other archaeologists—assume that a historical reality, or series of realities,[2] can be created by a fiction. That is, they believe that their taxonomic devices are actually various social units, or institutions, which once existed. Partly, this may be due to a search, among Americanists, for scientific respectability, for an obvious expression and extension of scientific method. The recurrent interest in systematics as a form of theory certainly suggests some preference for the archaeologist as scientist rather than as historian. It is all the more curious then, that American archaeology has failed to develop the techniques and methods of environmental archaeology and geochronology. The methodology required for such studies is certainly more rigorous and exacting than that required of classification, yet this is a field of study little understood by students of American prehistory. It offers, furthermore, new precision to archaeological analysis and historical criticism.

To take this particular volume as an example is perhaps unfair, but this is the most coherent, recent exposition, and, one may guess, the most influential. In brief, what the authors are doing, despite their opening statement, is to construct a set of forms which they then apply to aspects of New World history. This has been done with some interesting results,[3] but constitutes not much more, nor less, than a

1 Gordon R. Willey and Philip Phillips, *Method and Theory in American Archaeology* (Chicago: University of Chicago Press, 1958).

2 Or social, or political, or cultural realities, etc.

3 Not the least of these is the expert collation of many data on New World archaeology.

testing of tools. The developmental classification is simply a static representation of arbitrarily defined technological stages. It makes little difference that they are labeled Formative or Classic; they might just as well be called Mesolithic or Neolithic, or even given some other set of names for stages. No theory has been developed. What is *assumed* is an evolutionary theory about the nature of culture, though Willey and Phillips are shy about admitting this.[4] Moreover, no history has been written.

A theory should propose to account for the relationships of events and processes of American prehistory. Perhaps I should add "or portions thereof," since we are still a long way from being able to do this for the whole of the New World. An exposition which interprets events and causes involves thereby some theory as to the nature of history. Normally, the scale of such theories can be expected to be rather small. Archaeological theory must be developed in terms of particulars, as a means of explaining the significance of observable facts and inferable probabilities. To say it still another way: Theory should be developed in the process of writing history, not as a straightforward exercise in systematics.

What the archaeologist seeks is an understanding of human beings. He is a historian with particular sensibilities, whose scale at a given time is determined in good part not only by the nature of his sources, but by the adequacy of his techniques. The one constant factor is change, a factor which has not yet been satisfactorily handled. What usually happens is that the archaeologist appraises his primary sources by static means. His analyses of function or morphology, time, and association, are categorical. The categories are necessarily arbitrary, and may be named in different fashion. As an example, pointed tools, bifacially pressure-flaked, with a specific form, may be arrowheads, projectile points, or, simply, points. It makes little difference, for interpretation, whether they are classified in this way or that, whether by numbers, 1, 2, 3, etc., as Eden points or Folsom points, triangular with convex, or excurvate edges, etc., so long as the usages are consistent. The implement, the type of site, the range in time—relative or absolute—and the associations of, say, points with arrowshafts or knife handles, must all undergo a metamorphosis at the mind of the archaeologist. Again, it is perfectly possible to use phases in several different ways. A phase may be given a geographic name, as in the Southwest (e.g., Santa Cruz phase in Hohokam culture), in which case it is necessary to define it in space, particularly, and time through extended excavations in one region, where some continuity of culture in space may be expected. Or a phase may be treated as a site phenomenon, as in British Columbia (e.g., Locarno

4 *Op. cit.*, pp. 70-72.

Beach I), in which case expectations of continuity in space tend to be slighted in favor of continuity in time. For history, these variations have little real importance. They just make it easier to understand the form and relationships of some data one way or another. The taxonomic devices serve as points of reference and facilitate comparison, but they do not possess indefinite flexibility. They are not universals, and we should not stop our archaeology there. This is where we *begin* our history, initiate our explanations, our theories as to what happened and why. The object remains whether we classify it this way or that, and so does a site, its features —houses or middens or whatever (if our records are good)—and it remains worthless, no matter how well classified, until we take the step of using it for historical purposes. Categories are not historical, do not interpret or "exhibit" ideas; they are only methodical. One may even ask whether, as inductive method should, they lead to a questioning of the premises at the base of any propositions about the development of culture.

The Willey-Phillips classification attempts to add another dimension beyond simply organization for comparative purposes, that is, social reality. To claim that a taxonomy, either *a priori* or empirical, conforms to social or cultural realities is to misunderstand the function of such a tool. It is also to be mistaken about the nature of culture. If units of classification are specific social units, then culture is presumably the product of the archaeologist, rather than the subject of his interpretations. Correlations must then be the result rather than culture history. I suspect that many Americanists, in practice, act as if this were true. And it may be so, though I am inclined to doubt it. The equivalent of phase is not society, nor should it be. Society exists wherever men live together, but it does not exist in a taxonomic device. The phase represents the available data concerning some portion of a society or a culture, and is constantly subject to revision as additional information is acquired. If a phase, or other unit, is defined according to information gained from excavation of a village, we know only that this is one kind of sociopolitical unit present. We have not established that the phase has the reality of a village. This village may have been incorporated in other functioning social units—or dissected by them—and it is only an aspect of culture. It is important to develop means of recognizing various social units, but it is not important to conceive of the phase as having some social reality. No taxonomic unit can approximate the conditions of life, the habits and attitudes of people, their relationships, the changes which affect them, and the values they hold, all of which are part of social reality. The reasons should be obvious.

This approach leads, for example, to this curiosity: the treatment of horizon styles as indicators of social solidarity.[5] Presumably they may also suggest trade, cultural uniformity, a particular and limited use for such an item, a limited number of specialists producing them, and possibly other cultural, or social, features as well. If I understand correctly the functioning of some societies, at least, then social solidarity depends upon consciousness of kind. Can this be diffused over a wide area, through several communities? Can we assume sufficient similarity in subjective attitudes to permit inferring social solidarity as the complement of trade or diffusion of material goods? Perhaps this can be demonstrated in particular instances, but it can not be assumed in advance for all horizon styles. This is by way of saying that taxonomies, especially universal ones, explain little.

What is needed in American archaeology is not "taxonomic theory," but historical writing. Of this, there is astonishingly little,[6] perhaps in part because of the emphasis on taxonomy. Classification is a simplified way of expressing space and time relationships, and is a useful step in understanding the facts of an area under examination. What a classification does not provide, however, is an understanding of how and why. It does not define historical problems. Of what value is it, for example, to study prehistoric culture in the Plateau if our assessment is that culture there was at an Archaic, or Formative (or other) stage? I suggest that we need to know how and why long-standing cultural elements were reshaped to develop the historically known pattern of Plateau culture. Were the causes cultural and/or environmental, not only in general, but in particular ways? Did the trade in maritime shells influence the formation of villages in the central portions of the Plateau? Or did the development of villages, with resulting concentrations of people, permit the expansion of trade into a new area? There must be similar kinds of problems that need solution in other areas of New World prehistory, and I suggest that the probable answers to such questions do not lie in taxonomy. The realities of the past are not likely ever to be apprehended on a very large scale, although improvements in technique continue to enlarge the dimensions of prehistoric archaeology, but they are more likely to be understood through historical criticism than through classification. I suspect that there has not been enough "particularism" in American archaeology, i.e., meticulous study of areal and temporal microcosms, even if "in no other large part of the world does archaeology

5 *Idem*, p. 55.

6 Where, for example, is there a *Rome Beyond the Imperial Frontiers*, or a *Birth of Civilization in the Near East?* Surely, enough archaeology has been done in *some* parts of the United States to permit such writing.

stand so completely on its own feet as in the New World."[7] If this ringing phrase is true—and some Englishmen and Scandinavians may wish to challenge—there remains some growing to be done. Perhaps—at the moment—what needs most to be said is that when sufficient information is available, history will be written without reference to the taxonomy which classified the data.

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7 *Idem*, p. viii.

THE INCA OCCUPATION OF THE SOUTH COAST OF PERU

DOROTHY MENZEL

THE OVERWHELMING MAJORITY of historical documents dealing with Inca history and culture refer to events and conditions around Cuzco, the Inca capital. If we are interested in the cultural situation which the Incas found in the provinces they conquered and in the effect which their rule had on it we are lucky to find a few scraps of information on any one area. Archaeological investigations, on the other hand, can throw a flood of light on such problems. The possibilities can be illustrated from recent work on the south coast of Peru.

The part of the south coast with which we are concerned in this paper is the area between Chincha and Yauca; in terms of modern political units, the whole of the Department of Ica and the western extremity of the Department of Arequipa. This area is a desert plain with some low hills, crossed by a series of rivers which make irrigation-farming possible. From northwest to southeast the river valleys are Chincha, Pisco, Ica, Nasca, Acarí, and Yauca. Nasca is not a single valley unit like the others, however; it forks into several nearly parallel ravines of which the largest are Huayurí, Río Grande-Palpa, Ingenio (called Collao in ancient times), Nasca proper (the ancient Caxamarca), Taruga, and Poroma. Due to peculiarities of the local topography, the irrigated areas in Ica and the ravines of Nasca are located inland, at the foot of the mountains, rather than at the mouths of the rivers as is more usual in Peruvian coastal valleys.

No sharp natural or cultural boundary separates the valley of Chincha from the valleys of Cañete (ancient Huarco and Runahuana), Asia, and Mala, the next valleys going up the coast to the northwest. At the other end of the area we are considering, however, there is a sharp break; beyond Yauca the Andean foothills reach out to the sea, natural conditions are quite different, and there are important differences in the archaeological picture as well.

The historical sources which mention the south coast are not particularly helpful, except in so far as they provide us with a few dates. To begin with, no chronicler gives an explicit account of the political organization of the south coast at the time it came into contact with the Incas. It can, perhaps, be inferred from Cabello Balboa's statements about common policy toward the Inca invaders that there was some sort of political connection between the valleys of Mala, Cañete, and Chincha, but the connection need have been no more than a temporary military alliance. Cabello makes no mention at all of Pisco. The valleys of Ica, Nasca, and

Acarí are named together, but perhaps only because they offered no serious resistance to the Incas.[1] The authors of the *Relación de Chincha* of 1558 state that the valleys of Cañete, Chincha, and Ica each had its own ruler at the time of the Inca conquest, but no further details are given.[2]

The south coast had its first contact with the Incas early in the reign of Pachakuti (1438-1471) when, following the successful outcome of the Chanca War, the Inca ruler dispatched a raiding party under his brother, Qhapaq Yupanki, to visit the south coast valleys. The tradition of this contact was preserved at Chincha.[3] This event probably took place about 1440 according to Cabello Balboa's chronology.[4] No permanent conquests resulted from this raid, however.

The attention of the Inca rulers was distracted by campaigning in the north and around Lake Titicaca, and it was not until after Thupa Inka's expedition to Chile that a serious attempt was made to subjugate the south coast valleys. The conquest is described briefly by Cieza de León, Cabello Balboa, and Castro and Ortega Morejón. These authors agree that Ica, Nasca, and Acarí submitted peacefully, but there is some disagreement in the native tradition regarding the conquest of Chincha. Cieza says that the people of Chincha and the Incas of Cuzco were in agreement that Chincha also submitted peacefully, but that people from some of the other provinces claimed that the Chincha valley resisted. This latter tradition is reflected by Cabello.[5] According to Cabello Balboa's chronology, the conquest must have taken place about 1476.[6]

Direct Inca rule came to an end on the south coast in 1534, when Pizarro dispatched a party to found a Spanish settlement at Zangalla in the valley of Pisco, a place now known as Lima la Vieja, and granted the natives of the region in encomienda to the settlers of the new town. The town was abandoned when Lima was founded early in the following year, but the encomienda system was not interrupted.

This brief outline includes virtually all of the specific information about Inca rule on the south coast which can be gleaned from the historical sources. Now let us turn to the archaeological record and look for evidence reflecting the impact

1 Cabello Balboa, pt. 3, ch. xvii; 1945, p. 320.

2 Castro and Ortega Morejón, 1936, p. 236.

3 Cieza de León, pt. 1, ch. lxxiv; 1922, p. 247; pt. 2, ch. lix; 1943, p. 272; Castro and Ortega Morejón, 1936, p. 237.

4 Rowe, 1945, pp. 270, 279.

5 Cieza de León, pt. 2, ch. lix; 1943, pp. 273-274; Castro and Ortega Morejón, 1936, p. 237; Cabello Balboa, pt. 3, ch. xviii; 1945, p. 320.

6 Rowe, 1945, pp. 271-272, 279.

which Inca culture had on the area and the degree to which native traditions survived the brief period of Inca rule. The data available at present include the results of intensive studies of the Uhle collections from cemeteries in Chincha and Ica, preserved in the University of California Museum of Anthropology, information collected as part of the program of the Fourth University of California Archaeological Expedition to Peru in 1954-55, and subsequent survey work by a number of workers.[7] The information so far assembled refers principally to architecture and pottery. It is far from complete, but there is enough to permit some useful interpretations.

ARCHITECTURE

Among the occupation sites of the Late Horizon, the period of Inca rule on the south coast, it is possible to distinguish a group of ruins which, because of their peculiarities of plan and construction and their association with Inca pottery, can be identified as centers of imperial administration. Most other Late Horizon habitation sites on the south coast show little or no evidence of Inca influence and belong rather in the native cultural tradition of the valley in which they are found. This contrast suggests a promising line of investigation.

The Inca administrative center in the Chincha valley appears to be a building complex which forms part of the site of La Centinela, near Tambo de Mora.[8] La Centinela is an extensive group of ruins of tapia construction in the native Chincha tradition, and much of it was probably built before the Inca conquest. It is the largest and most imposing site in the valley, and Dwight T. Wallace discovered in 1958 that it was the hub of a system of straight roads radiating across the valley. The pattern of building complexes and roads attributable to the native tradition in Late Intermediate Period and Late Horizon times suggests that Chincha had a highly centralized administration before the Incas came, and that the Incas took over the native capital and administrative machinery when they conquered Chincha.

The situation in Pisco is somewhat different. There are at least two big Inca sites in this valley, Lima la Vieja, below the modern town of Humay, and Tambo

7 My participation in the program of the Fourth University of California Archaeological Expedition to Peru in 1954-55 was financed in part by a grant made by the Wenner-Gren Foundation for Anthropological Research for expedition expenses and in part by Mr Victor W. Von Hagen's Inca Highway Expedition. Another field trip in 1958 was made possible by a grant from the American Philosophical Society. This support is gratefully acknowledged. I want to express my thanks to Francis A. Riddell, David A. Robinson, Dwight T. Wallace, John H. Rowe, and Seth Leacock for unpublished information on south coast archaeology, and to John H. Rowe and Eugene A. Hammel for advice in the preparation of this paper.

8 Uhle, 1924, pp. 70-80, plates 1-4; Lothrop, 1926.

Colorado above it, and neither is associated with buildings in the native tradition comparable to those of La Centinela.[9] The archaeological evidence suggests that the Incas administered this valley in a series of separate units, and the situation may reflect a lack of centralized administration before the Incas came.

In the Ica valley, the center of Inca administration seems to have been the site of Old Ica in the *pago de Tacaraca* about 10 km south of the modern city of Ica. Old Ica is a dispersed cluster of monumental buildings, many built on mounds or raised platforms, with little or no evidence of habitation in the spaces between them. The pottery associated with the buildings of Old Ica suggests that construction of the site started not later than the beginning of the Late Intermediate Period. The site is the most imposing one in the valley, and it was probably the capital of Ica at the time of the Inca conquest. In one section of Old Ica there was apparently an Inca building complex, identifiable by construction details and surface sherds. At Soniche, one of the cemeteries adjacent to the site of Old Ica, Uhle excavated a number of elaborate burials of the Inca period containing a wealth of imported objects. Elsewhere in the Ica valley traces of Inca occupation are very scarce.

In the ravines of Nasca two major Inca centers have been identified. One is the Tambo de Collao, in the ravine of Ingenio, and the other is Paredones, the ancient Caxamarca, on the opposite side of the river from the present town of Nasca. Neither of these sites is associated with a major occupation referable to the native tradition. Further survey work in the Nasca drainage may lead to the identification of other Inca centers.

In Acarí the Inca administrative center was at Tambo Viejo (Old Acarí), 3 km south of the modern town. The Inca building complex at Tambo Viejo was built on top of the ruins of a very extensive walled town dating from the early part of the Early Intermediate Period (Nasca 3). There is no evidence for any really imposing late site of the native tradition anywhere in the valley which might have served as the capital of a centralized government before the Inca conquest.

In Yauca the Inca center was at the Tambo de Jaqui on the Hacienda Lampilla. Again it is not associated with any major construction in the native tradition.

The available archaeological evidence, therefore, suggests that the valleys of Chincha and Ica had some pattern of centralized authority at the time of the Inca invasion and that the other valleys did not. The evidence for centralized government is very strong in Chincha and less so in Ica, where the site of Old Ica could have been a center of religious rather than political authority, because the major

9 For plans and photographs of Tambo Colorado see Urteaga, 1939, and Kroeber, 1944, plates 18-20.

structures are on artificial mounds. The Incas took advantage of native centralization where it existed, constructing their own administrative headquarters in the native capital; elsewhere they established a new center, sometimes, as in the case of Acarí, taking advantage of an abandoned site which had dominated the valley in much earlier times.

The building complexes identified as Inca administrative centers share a number of distinctive features of plan and construction, although no two are exactly alike. The most obvious common feature is the arrangement of substantial buildings of rectangular plan around a plaza. It is a formal layout, formal in the sense that it was laid out on a preconceived plan. It is possible that excavations at Chincha would reveal that some of the late sites in the local tradition were also built on a regular plan, but elsewhere on the south coast the plans of sites in the native tradition are far less regular than those of Inca sites.

The central plaza in the Inca centers varies from rectangular to trapezoidal. On one side is a large rectangular building consisting of a complex of small rooms and enclosed courts built with special care and often having special decorative features, such as niches or wall painting. Less elaborate buildings may be found on the other sides of the plaza, but one side is often left open. There is a low platform in front of the principal building unit, and sometimes on other sides of the plaza as well. The principal building is built on a slope on the uphill side of the plaza at Tambo Colorado, Tambo de Collao, and Paredones; at Tambo Viejo and Tambo de Jaqui it is set on the edge of a bluff overlooking the river. At Tambo Viejo another principal building is built on a raised platform, and the Inca highway led directly to it.

A certain type of large rectangular adobe with a smooth surface is used in varying amounts in the construction of all the formal Inca centers. These adobes differ in shape, size, and temper from Inca adobes in the highlands, so it must be supposed that the Incas derived the coastal form from some local tradition. The architecture of Chincha is unlikely to have served the Incas as a model, since the prevailing construction material at Chincha was tapia. If the coast type of Inca adobe was derived from any south coast tradition Ica is the best candidate, for rectangular adobes were used in the construction of Late Intermediate Period sites in Ica which were not occupied in the Late Horizon. The Ica tradition adobes are less regular than the Inca ones, however, and are laid up with more clay between the courses.

The use of adobes in Inca buildings on the south coast did not preclude the use of other construction materials as well, especially field stones from the vicinity of the respective sites, used mainly for foundations. Cobbles from the adjacent river

bed were used at Tambo Viejo in Acarí. Dressed stone construction in the Inca style is very rare on the coast, but some dressed stone was used in wall foundations at Paredones. On the whole, the proportion of adobes used is highest in the more elaborately decorated buildings.

Inca architecture on the south coast shows expectable parallels with Inca adobe construction in the Cuzco area, but there are also some interesting differences. The coast buildings share with the highland ones the use of adobes on stone foundations, lintels of wooden beams covered with braided rope, battered supporting walls, trapezoidal niches and doorways, and plastered wall surfaces. The first Spanish invaders reported seeing painted adobe walls at Cuzco, and some paint is preserved on the walls of Inca buildings on the coast. The walls of Tambo Colorado are extensively painted in red and yellow, while one room of the principal Inca building at La Centinela has the remnants of a fine Inca geometric design painted in black and red on white. On the coast, as in the sierra, Inca buildings consist characteristically of groups of connecting rooms which have only a single common access to the outside for each group. On the other hand, gable roofs, which are common in the sierra, do not occur on the south coast, and there is a frieze of adobe reliefs at Tambo Colorado which has no known sierra prototype.

Our best information about the relationship of the Inca administrative centers to contemporary settlements of the native tradition comes from the valley of Acarí, where several native settlements dating to the Late Horizon were explored by the University of California expedition in 1954. Three important foci of native settlement were located in this valley. One of these foci consists of four clusters of houses scattered on the plain within 500 meters of the Inca administrative center at Tambo Viejo. Only low wall foundations of river cobbles remain. These foundations outline small courtyards surrounded by smaller rooms and storage pits in irregular fashion, some of the units being separated by narrow alleys. The upper walls of the houses were probably made of matting or of wattle and daub, the common construction materials used today for dwellings in this area. The arrangement of these four clusters of houses suggests that they were satellite communities of the Inca center, established to provide convenient sources of labor for the representatives of the Inca government. No larger building such as one might expect a native noble to have occupied is found in these clusters. The second major focus of native settlement is the site of Sahuacarí, located on a steep slope on the edge of the valley across the river and slightly above Tambo Viejo. Sahuacarí is built on terraces and consists of compounds of rectangular houses of varying sizes and narrow rectangular storage bins. The compounds are separated at intervals by narrow streets. The walls are built of angular field stones laid in mud, and some of

them are preserved to their original height of two to three meters. Some of the houses have small interior niches and pegs protruding from the walls, features which strongly suggest Inca influence. The third major native site is Otaparo, located on a prominent hill about 20 km up-valley from Tambo Viejo. The buildings at Otaparo are poorly preserved, and only the stone foundations can be traced. The pottery, however, indicates that the site belongs to the period of Inca occupation. Both Sahuacarí and Otaparo are more ambitious settlements than the clusters of houses near Tambo Viejo, but neither has any truly monumental buildings.

The settlement pattern situation in Acarí may be summarized as follows. There was no monumental site built in the native tradition comparable to La Centinela in Chincha and Old Ica. Sahuacarí and Otaparo, the largest concentrated native settlements, are no more than good sized villages, without imposing monumental buildings. The Inca administrative center appears to have attracted some local people who were settled in four hamlets around it.

The clustering of informal settlement units around an Inca administrative center is found only at Acarí. In the other valleys, native villages are not directly associated with the Inca centers, and the native tradition of settlement pattern and domestic architecture survived without any traceable Inca influence. In each valley the situation is different in detail, however, as the native traditions differed in type of site preferred, arrangement of buildings, and construction methods. In Chincha, Pisco, and Ica there were also monumental structures of the native tradition, but it is not yet clear whether all of them simply survived from before the Inca conquest, or whether new ones were built in the period of Inca rule. Comparable monumental structures in the native tradition have not been reported from Nasca and Yauca, as they have not been found in Acarí.

POTTERY

At the time of the Inca conquest there were four native styles of pottery in the south coast area with which we are concerned: Chincha, Ica, Poroma, and Acarí. There were, that is, fewer separate traditions of pottery than there were valleys.

The Chincha style had its home in the valley of Chincha. It seems to have been more closely related to the style of Cañete than to other south coast styles, although it traded some influences with the style of Ica. Occasional imports from Chincha are found in Pisco, and imitations of Chincha vessels occur in Ica.

The Ica style was the most highly decorated style in the south coast area and, at the same time, the one which had the most influence on other styles.[10] It was the

10 Menzel, ms.

style not only of the Ica valley but of the valley of Pisco and the ravine of Huayurí as well. In the Late Horizon two regional variants developed in the Ica style. One of the variants is found in the main part of the valley of Ica, down to Ocucaje, and in Pisco; the other is found at Cayango and other parts of the Ica valley below Ocucaje and in Huayurí. Ica pottery was imitated in Inca times as far north as Chincha and as far south as Acarí at least.

Poroma is the name given to the Late Intermediate Period and Late Horizon style found in the ravines of Nasca south of Huayurí. It is best known from collections made in the ravine of Nasca, and it might appropriately be called Nasca except for the fact that this name is already in use for an earlier style.

The Acarí style is found in the valleys of Acarí and Yauca, and occasional trade pieces turn up even farther south. This style is obviously related to the Poroma style, though not identical to it.

Our information is not sufficiently complete to permit an equally thorough analysis of the effect of Inca rule on all four of these local south coast styles. We have the most detailed chronology and the best record of archaeological associations for the Ica tradition, some information of this sort for Acarí and Chincha, and very little for the Poroma tradition. It will be convenient to discuss the four situations in the order of the completeness of the record rather than in their geographical order. We begin, therefore, with Ica.

The Uhle collection from Ica in the University of California Museum of Anthropology includes lots of material from four burials representing the period just before the Inca conquest, thirteen which can be dated to the period of Inca rule, and ten burials of the early Colonial Period, some of which contain Spanish glass trade beads. On the basis of the grave associations in these burials, Ica pottery of the native tradition can be sorted into three stylistic phases, each one corresponding to one of the time periods represented by the graves. The pre-Inca phase is named Soniche, after the principal cemetery at Old Ica which Uhle excavated, the phase corresponding to the Inca occupation is called Tacaraca A, and the early Colonial phase is called Tacaraca B.

In the burials corresponding to the period of Inca rule three additional types of pottery, more or less foreign to the native tradition, may be found alongside of pottery of the Tacaraca A phase. These three additional types are:

1. Inca style, including some examples so characteristic of the Inca tradition that they are probably imports from the Cuzco area and others which are certainly local imitations of vessels of Cuzco type.

2. Ica-Inca style, a peculiar style of local manufacture which includes some Inca features and, rarely, some derived from the Ica tradition. Most of its features,

however, cannot be traced to antecedents in either tradition. There are two common shapes, a barrel shaped jar[11] and a small flat-bottomed bottle with a loop handle.[12] Inca influence can be traced in the handles and spouts of these vessels but is largely absent in the painted designs. The designs have only vague antecedents in the Ica tradition, and thus represent essentially new creations. The Ica-Inca style, more than any other, characterizes the period of Inca rule at Ica, and Ica-Inca design elements are occasionally found on local imitations of Inca vessels and on vessels of the native Tacaraca A style.

In addition to the characteristic jar and bottle, there is a type of plate which, in the Ica valley, has much the same relation to the Ica and Inca traditions of pottery-making that the containers do. This plate has a standardized shape and is usually decorated with a painted bird design, for which reason the name "bird plate" seems appropriate. As far as its associations in Ica are concerned, the bird plate can be treated as simply a third Ica-Inca vessel type. It has a different distribution pattern from that of the Ica-Inca jar and bottle on the rest of the south coast, however.

3. Chimu style, either in the form of specimens imported from the north coast or as local imitations. The commonest Chimu style specimens are double-bodied whistling jars of smoked blackware with modelled bird heads at the top of the spouts. These vessels are usually found in pairs, one pair to a tomb. Blackware stirrup-spout bottles are also found.

There are some interesting differences in construction and contents among the burials of the Inca period excavated by Uhle at Ica. Some of the burials are large and carefully arranged, with an abundance of grave goods, including objects such as gold and silver ornaments and, in one case, a wooden stool, which is known from historical sources to have been a symbol of rank the use of which was restricted by the Incas to the higher nobility. Other burials are smaller and more informal and lack known symbols of rank. Pottery belonging to the Inca, Ica-Inca, and Chimu styles is concentrated in the larger and more elaborate tombs, although these usually also contain some Tacaraca A specimens; the smaller tombs may contain Tacaraca A style vessels only, or sometimes a mixture of Tacaraca A and Ica-Inca pieces. The concentration of exotic pottery in the larger tombs is impressive. As many as half the vessels in a large grave lot may be Inca and another third Ica-Inca style.

Surface collections from habitation sites in the Ica valley give quite a different picture. Inca and Ica-Inca sherds are concentrated in the section of Old Ica which

11 Kroeber and Strong, 1924b, pl. 38, d-f; pl. 39, a, e.
12 *Idem*, pl. 40, e, n.

served as the Inca administrative center. Elsewhere they are very rare, and most of the small habitation sites of the period of Inca rule yield pure Tacaraca A sherds.

The burials of the early Colonial Period present a marked contrast to those of the period of Inca occupation. The early Colonial burials are noticeably poorer, both in luxury goods and in numbers of pottery vessels. Inca influences are completely absent from the pottery. No Inca or Ica-Inca vessels occur in these burials, the only pottery found belonging to the Tacaraca B phase of the native Ica tradition. At the same time, the only sign of Spanish influence takes the form of rare trade beads. The Tacaraca B style is in part an expectable development out of Tacaraca A, but it also includes the deliberate revival of many features of the Soniche style which are absent from pottery of the intervening Inca period. In many respects, Tacaraca B pottery looks more like Soniche pottery than Tacaraca A does, a situation which has created understandable difficulties in earlier interpretations of the Ica evidence, including Uhle's.

The evidence of pottery in the Ica valley, then, suggests that direct Inca influence after the Inca conquest was felt most strongly in the upper ranks of the native nobility and in the immediate vicinity of the Inca administrative center. Among the ordinary inhabitants of Ica and in the settlements more remote from the Inca control point there is as little sign of Inca influence in pottery as there is in architecture. The potters working for the nobility, however, not only imitated Inca vessels directly but developed a new and original style of their own which owes remarkably little to the earlier native tradition of Ica. After the Spanish conquest, all elements of decoration associated with Inca rule were swept away, and an attempt was made to return to the "pure" native style. The fact that the Inca dominion had only lasted for about sixty years probably facilitated this striking reaction.

The archaeological associations in which the various styles of pottery are found in the Ica valley permit some inferences regarding prestige differences between the styles. During the period of Inca rule, the Inca style had, as we might expect, the highest prestige at Ica, a prestige inferable from its associations with the nobility and the imitation of Inca decorative features in the locally made styles. The Ica-Inca style was next in prestige, to judge from its abundance in the richer burials. The fact that the Ica-Inca style was eliminated at the time of the Spanish conquest in the same way as the Inca style suggests that the prestige of Ica-Inca pottery was closely related to Inca prestige in spite of the fact that the Ica-Inca style was essentially a local creation. The Chimu style vessels found in the tombs were imports ranking with Ica-Inca pottery in prestige at Ica. The native Ica tradition

had the lowest prestige in the society of the capital but maintained its strength in the rural areas.

The pattern of distribution of pottery styles in Inca period Ica fits the normal pattern of distribution of customs in a social hierarchy, that is, that radically new features are taken in at the top of the hierarchy, while more conservative ones persist among people at the bottom, with rural areas associated with the lowest levels of the hierarchy. There is thus clear archaeological evidence for the existence of a fairly elaborate social hierarchy at Ica in the period of Inca rule.

The distribution of Ica valley pottery styles elsewhere on the south coast during the period of Inca occupation also offers some points of interest. Ica-Inca fragments are rare, but they do occur in Chincha and other valleys near Ica. Vessels of Cuzco Inca shape with Ica-Inca designs are not uncommon in museum collections and are said to come from a number of other places besides Ica. Occasional specimens in the Ica-Inca style may have been traded even more widely, for there is a small jar in the Bandelier Collection in the American Museum of Natural History in New York which is supposed to come from the Lake Titicaca area.

While Ica-Inca jars and bottles are not common outside of the Ica valley, bird plates are found in the Nasca region and in the valley of Acarí in about the same proportion as at Ica, and with no apparent stylistic variations. The bird plate has the same abrupt origin and Inca associations in these other valleys that it does in Ica.

Sherds representing vessels of the more traditional Tacaraca A style occur in small but regular percentages in the Chincha, Nasca, and Acarí valleys, and decorative traits derived from the Tacaraca A style are found as far south as the Chala area, although there in a very diluted form and mixed with a larger component of Inca influence. In the Nasca and Acarí valleys, too, traditional Tacaraca A shapes are often associated with Inca or Inca associated traits of paste, shape, and pigmentation.

In contrast to this occasional influence, there is one Tacaraca A vessel, a shallow dish (Dish C shape in the Ica classification), which is found as commonly at Nasca and Acarí as at Ica. It appears to have been incorporated into the respective local styles as a standard type. This dish shape is found in both painted redware and smoked blackware at Ica, while at Nasca and Acarí it occurs predominantly in smoked blackware. The details of its shape in the Late Horizon styles of the Nasca and Acarí valleys are such that it must have been newly diffused from Ica during the period of Inca rule and could not have developed independently in the southern valleys from some variety which diffused earlier. The Dish C shape is not associated with Inca traits, either at Ica or elsewhere.

These distributions and associations convey the impression that, north and south of Ica, all Ica-made pottery, even the native Tacaraca A style which has the lowest prestige associations at Ica, enjoyed some prestige. There is abundant evidence that the previous phase of the Ica pottery tradition had a very influential relation to its neighbors in the period just preceding the Inca conquest, so the Inca period situation is in part simply a continuation of something much older. Except in the case of the adoption of the Dish C shape, the evidence suggests that the prestige of Ica pottery was in some way associated with Inca prestige. There is no historical evidence which might help to explain this situation. We can only speculate that the Incas accorded Ica some sort of preferential treatment in the south coast area, perhaps subordinating their administration of some of the other valleys to a governor at Ica, as the Spanish conquerors did later, or using the Ica nobility as agents in other valleys.

Next to Ica, our fullest evidence for Inca period pottery comes from the valley of Acarí. The evidence is not precisely comparable to that from Ica, because no tombs of the Inca period have been excavated at Acarí. The evidence in the latter valley comes rather from excavations in the midden at the Inca administrative center of Tambo Viejo, conducted by Francis A. Riddell and me, and from surface collections at other sites.[13] In the excavations at Tambo Viejo an attempt was made to distinguish between the pottery of the Inca occupation and that of the early Colonial Period. No distinction could be drawn in the native pottery, in spite of the fact that the occupation of the site was continuous between the two periods and that a few artifacts of Spanish origin were found in the upper levels of the midden. Evidently there was no abrupt reaction to the fall of Inca power at Acarí such as we noted for Ica.

As noted earlier, Acarí had a native pottery tradition of its own, most closely related to Poroma, and this tradition continued into early Colonial times. The pottery in the native tradition is classified in two types, Acarí Polychrome and Otaparo Polychrome, neither of which shows any significant Inca influence.

Pure Cuzco Inca pottery and local imitations of it occur in small proportions in all parts of the Inca period site at Tambo Viejo but are very rare elsewhere in the valley. The local imitations are considerably more common than the actual imports.

Acarí has an original local style corresponding in its relations to other styles to the Ica-Inca pottery of the Ica valley. It can appropriately be called Acarí-Inca. The most common type of Acarí-Inca pottery is Sahuacarí Polychrome, and it includes a jar shape and a plate. The painted designs of Sahuacarí Polychrome

13 Rowe, 1956, pp. 137-138.

include a number of features which have no immediate local antecedents. One is a checkerboard design which resembles a Qoripata Polychrome pattern at Cuzco.[14] Another common feature is a ball band design which is derived from the Poroma tradition.[15] Then there is a dotted horseshoe design identical with a pattern found on Middle Horizon pottery in the Acarí valley but not occurring in other later styles. Another new design is an eight-pointed star which seems to be borrowed from the pottery tradition of the Ocoña and Majes valleys far to the south. Sahuacarí Polychrome is thus a remarkably eclectic style.

As noted earlier, the bird plate is also common at Acarí, where it is associated with the Acarí-Inca style more or less the way it is associated with Ica-Inca in the Ica valley.

There is another peculiar complex of features at Acarí in the period of Inca rule which is closely associated with Inca influence without being a direct imitation of Cuzco models. The complex consists of a peculiar type of white paste with very fine crystalline white temper and occasional fine reddish specks in it, and a series of painted designs, notably a catfish design which seems to be at home in the Titicaca basin. The white paste also suggests a relationship with the Titicaca area, for white paste is a characteristic of the Inca-associated Taraco Polychrome style in Puno.[16] At Acarí and Nasca white paste is very often used for bird plates, and sometimes it is the base for Inca and Acarí-Inca vessels and for Acarí imitations of the Tacaraca A style. The catfish design is found on Ica tradition shapes at Ica as well, but no white paste specimens have yet turned up in the Ica valley.

The distribution of the Acarí styles of the Inca period does not exactly parallel the distribution of the styles of Ica. Acarí Polychrome is the most abundant type and is found all over the Acarí valley and also occasionally in the Nasca drainage and at Chala. Otaparo Polychrome is less abundant but is also found all over Acarí and sometimes in the Nasca drainage. Acarí-Inca pottery, including Sahuacarí Polychrome and bird plates, is not restricted to the Inca administrative center, the way Ica-Inca pottery is in Ica. It is found in small percentages on most of the Inca period sites in the Acarí valley and is not concentrated at any one particular site. However, it has not been found in the Nasca drainage or anywhere south of Yauca.

The distribution of Acarí pottery styles suggests that Acarí had a relatively modest prestige position among the south coast valleys in Inca times, and that it had a less elaborate social hierarchy than Ica. Inca influence was probably no greater than at Ica, but it seems to have been somewhat more evenly distributed through the population.

14 Rowe, 1944, p. 48-49.

15 Robinson, ms.

16 Tschopik, 1946, pp. 31-32.

The principal evidence for Chincha pottery in the period with which we are concerned is still furnished by the collections made by Max Uhle at the turn of the century as a result of his excavations in Chincha cemeteries.[17] The pottery from Uhle's excavations which belongs to the native tradition of Chincha can be separated into two stylistic units which, for the purposes of this discussion, we can call Chincha and Derived Chincha. The Chincha phase represents the phase of the local tradition which was flourishing at the time of the Inca conquest, while the Derived Chincha phase corresponds to the period of Inca rule and continues into early Colonial times. Derived Chincha pottery is associated in the graves dug by Uhle with imported Inca vessels, local imitations of Inca pieces, a Chincha-Inca style of composite origins, some Chimu style specimens, and a few imitations of Tacaraca A and Ica-Inca vessels.

The Chincha style of our discussion is Kroeber and Strong's Late Chincha I.[18] It is a highly characteristic local style including many well made and elaborately decorated pieces. The succeeding Derived Chincha pottery—no illustrations of which have been published—is an impoverished version of the Chincha style rather than a further development at about the same level of quality such as we noted at Ica. Derived Chincha vessels are relatively poorly made, and very few of them have painted designs. In the burials containing Derived Chincha vessels the more elaborate pieces are in other styles.

Chincha-Inca pottery is composed of a mixture of elements, including Inca influences and elements borrowed from other regional pottery styles, most notably, perhaps, from Pachacamac on the central coast. It may also include some older features from the Chincha tradition. The most common shapes are a small bottle[19] and a jar.[20] The Chincha-Inca style is a mixture of very much the same type as the Ica-Inca and Acarí-Inca styles which we have described.

Inca style pieces imported from Cuzco and local imitations of Inca pottery are so abundant in the graves dug by Uhle that Kroeber and Strong undertook to describe the Inca style on the basis of this collection.[21]

The Inca period cemeteries excavated by Uhle are all located in the immediate vicinity of La Centinela, the Inca administrative center, so they probably reflect the situation at the local capital. I have not had an opportunity to make the type of study of surface collections from other parts of Chincha on which reliable statements about distributions might be made. In spite of the area limitation, however, some interpretive observations may be made.

17 Kroeber and Strong, 1924a; Uhle, 1924.
18 Kroeber and Strong, 1924a, plates 11-12, fig. 6.
19 *Idem*, fig. 3a,b.
20 *Idem*, fig. 4c,d; fig. 9a,c,d.
21 *Idem*, pp. 9-16.

None of Uhle's tombs in the Chincha valley were as rich as the richest he dug in Ica. This fact does not mean that there were no very rich tombs in Chincha, of course; what it does mean is that our evidence probably relates to statuses farther down the social hierarchy than those at the top represented at Ica. In view of this fact the proportions of the styles in the Inca period graves is very remarkable. The Derived Chincha style represents only twenty percent of the total pottery found, while most of the rest consists of imitation Inca and Chincha-Inca specimens. Evidently the impact of the Inca conquest was more far reaching at La Centinela than at Old Ica. Inca influences penetrated down the hierarchy in greater strength, and the local pottery tradition lost more of its prestige. There was no anti-Inca reaction at the time of the Spanish conquest, either. There were Spanish glass beads in three of the burials excavated by Uhle, and their contents are not significantly different from those of the graves which lack evidence of Spanish contact.

The evidence for the Nasca drainage is especially meager. It consists chiefly of surface collections made at looted cemeteries and at a few habitation sites, plus observations made on whole vessels collected by Uhle in 1905 and Kroeber in 1926. There are no associations which permit chronological distinctions in the Poroma style and related materials. Nasca seems to have had the same range of styles and style mixtures which we noted for Acarí: its own native tradition, Inca style pieces, local combinations of Inca and other elements, and the widely distributed bird plate. We have also noted the presence of influences from Ica and Acarí.[22]

Even with our present unsatisfactory evidence for Nasca and the virtual absence of information for Pisco and Yauca, some general observations may be hazarded with respect to the reflection of cultural events in south coast pottery during the Inca occupation. In Chincha there seems to have been a very sharp break in the native pottery tradition about the time of the Inca conquest. Elsewhere, the local pottery traditions continued through the Inca period and into early Colonial times, the continuity being most marked in the small settlements away from the centers of Inca administration. At Ica the native tradition had such vitality that it became the basis for a spectacular artistic revival after the fall of Inca power, a revival which led to the purging of all features associated with Inca rule.

The Inca conquest brought some Cuzco pottery in trade to all of the south coast valleys, and Inca models were everywhere imitated by local potters. This fact could have been predicted without archaeological study. More interesting is the evidence for the development of a series of new eclectic styles. In each south coast

22 The fact that even this much information is available on the Nasca area we owe to the pioneer survey of David A. Robinson (ms.).

valley the local potters developed an original style which included some traditional elements, some Inca influences, an occasional feature borrowed from local pottery of centuries earlier, and traits from a variety of other areas, some apparently as far away as the Titicaca basin and Pachacamac on the central coast. All these new styles have much in common, but each is a distinctive combination of elements. These new styles were associated in people's minds with Inca rule and shared some of the prestige accorded to genuine Inca pottery.

CONCLUSIONS

Archaeological data provide much new evidence regarding the significance of the Inca occupation on the south coast of Peru. We have reviewed the evidence of architecture and pottery in some detail and can now summarize the cultural information which we have extracted from it.

There seem to have been marked differences in social and political organization among the south coast valleys at the time they were conquered by the Incas. Chincha had a powerful centralized government, while Pisco was probably divided. Ica had a centralized organization of some kind, but we cannot be sure whether it was political or religious in nature. There was, however, a concentration of wealth and prestige at the site of Old Ica. Ica also had more cultural prestige than any of the other valleys, and its decorated pottery was sought and imitated throughout the whole area. There is no evidence now available suggesting centralization in the Nasca drainage. Acarí seems to have had no central authority and no point at which there was a concentration of prestige.

The Incas took advantage of existing centralization in Chincha and Ica, building their administrative centers at the focus of native authority. They probably ruled through the native nobility in both valleys. In the valleys in which there was no centralized authority already, the Incas imposed their own, constructing an administrative center at some convenient point to serve as the focus of Inca control. These administrative centers were located on the Inca coast road, a fact which emphasizes the military and administrative significance of the road system. No effort was made in this area to concentrate the population in towns, and existing patterns of settlement were not seriously interfered with. Even in the case of Acarí, where the Incas encouraged or permitted the settlement of some groups of natives near their administrative center, the native houses are in small separate clusters, not laid out on a regular plan.

Inca influence can be traced most widely in those valleys like Acarí where there was no native centralization to start with and no highly organized native nobility.

In contrast, in Ica the native nobility succeeded in monopolizing whatever "advantages" Inca rule may have brought to the valley, and little Inca influence seems to have filtered down to the ordinary farmer. The Ica nobility supported the Inca regime enthusiastically as long as it remained powerful. When it disintegrated, however, the nobles of Ica reacted against their Inca tradition with extraordinary violence, unparalleled elsewhere in the area studied.

The Inca conquest did not simply bring a mechanical addition of Cuzco influence to south coast culture. It led to some original developments in monumental architecture and in pottery. The architectural innovations are seen in the Inca administrative centers, which were probably built under the supervision of Inca architects; a more or less uniform "coast Inca" style was developed. The new developments in pottery are more spectacular and involve the creation of important new local styles. These styles combine elements gleaned from a wide variety of pottery traditions and some which were probably outright inventions. The mixture is different in each valley, however, a fact which suggests that the new styles were the work of local potters working more or less independently. It is possible that the development of these new styles was deliberately fostered by the Inca government as a means of weakening local loyalties symbolized by the earlier native traditions; at any rate, the new styles were closely associated with the prestige of the Inca regime.

The amount of new information on the cultural significance of the Inca occupation on the south coast which we have succeeded in extracting from the odds and ends of archaeological information now available suggests the remarkable possibilities which this type of study offers. Instead of lamenting the absence of written records in the Andean area, we have only to look closely at the abundant evidence which has survived from the past in the form of archaeological associations. The answers to many of our questions can be found in the ground.

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BERKELEY, CALIFORNIA

MAGIC AND WITCHCRAFT: AN INTERPRETATION FROM DISSONANCE THEORY

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INTRODUCTION

IN COMMENTING recently on the few advances made in the social anthropology of religion during the past quarter century, David Aberle suggests that we might do well to build on what we have.[1] We might, for example, attempt to test "on a broad comparative basis" Malinowski's hypothesis "that magical action occurs in those important areas of life where available empirical knowledge provides inadequate prediction and control." We agree that it is worthwhile to nail down even a single proposition of this sort.[2] However, let us link this more limited task to the general proposition which Aberle makes in closing the above discussion:

> The affirmation of values and the search for control where control is difficult have one thing in common to me: a state of tension. Many but not all of these tensions involve conflict. They all involve some problem of discrepancy—of a gap between norm and performance, between wish and actuality, between desire to love wholly and feelings of hatred—and so on. It seems to me that both religion and magic attempt to cope with these tensions, whether or not one chooses to regard this as their core function.[3]

This statement implies a motivational source in the expression of magical beliefs and practices—a conscious or non-conscious need to resolve tension and personal conflict. However, it also involves a cognitive component; individuals may be aware that their techniques for coping with situations are not always consistent with what they know about the adequacy of these techniques. Festinger's recent theory of cognitive dissonance[4] suggests a useful frame of reference for treating the persistence, decline, or increase in the practice of magic and witchcraft. Dissonance theory provides a dynamic set of propositions which extends the scope of Malinowski's hypothesis that magic aims at technical control over the physical environment. If witchcraft and sorcery are treated as a variety of magic which aims at control over the behavior of persons, much of witchcraft and sorcery can be

1 Aberle, 1957, p. 6.

2 There are considerable data which indicate that, for a given phenomenon, the degree of technological control and the practice of magic vary inversely. For example, Malinowski, 1935, pp. 435, 444; Webster, 1948, pp. 392, 503; Evans-Pritchard, 1937, p. 438; Oliver, 1949, p. 21; Redfield, 1941, p. 145; Piobb, 1907, pp. 163-175.

3 Aberle, 1957, p. 9.

4 Festinger, 1957.

placed in the same category and explained in the same terms.[5] Specifically, we suggest that magic arises as a symbolic means of handling important environmental influences which are not subject to empirical control, and also for dealing with the anxiety, frustration, or threat which may result when people are confronted with important environmental forces which they cannot master. Because magic is, objectively, an imperfect mechanism of control, the use of magic often introduces dissonance—a state of tension—and people will seek to reduce this tension in ways which are predictable from dissonance theory.[6] In the remainder of this paper we shall, first, spell out certain relevant theorems of dissonance theory along with several additional explanatory concepts, and then bring selected case materials to bear upon them. The method of exposition is essentially clinical. An attempt is made to isolate certain critical dimensions of magic and witchcraft, in order to make them amenable to more systematic cross-cultural testing.[7]

I

Let us now attempt to state those aspects of dissonance theory with which we will be concerned here. "Dissonance" may best be defined as a special relationship between cognitions, or clusters of knowledges: in a dissonant relationship one cognition affirms what another cognition denies. If cognitive element(s) (1) affirm "A," and cognitive element(s) (2) affirm "not-A", we have a dissonant relationship between item or cluster 1 and item or cluster 2. It will be seen that "dissonance" refers to a "non-fitting" or incongruity between cognitions, or clusters of cognitions. Any two cognitions or cognitive clusters may be related in several

5 If one defines witchcraft and sorcery as aspects of magical practice primarily designed to control human objects, one would deduce that the degree of socio-political control in a society and the degree of existing, or imputed, witchcraft and sorcery vary inversely. The Kwoma illustrate a group where a relatively ineffective socio-political organization is joined to high incidence of fear of sorcery. All disease is ascribed to sorcery; anyone caught stealing sorcery material may be killed on the spot. Whiting concludes that: "The belief in sorcery, as well as furnishing an explanation for sickness, functions as a means of social control. It stands, indeed, in lieu of a police force and strong political organization as a preventive of criminal behavior" (Whiting, 1941, p. 219). Also see Opler, 1955, pp. 193-196; Hogbin, 1951, p. 144. Among the Navaho: "Headmen have no powers of coercion, save possibly that people sometimes fear them as witches" (Kluckhohn and Leighton, 1946).

6 By concerning ourselves with this cognitive-motivational dimension of magic and witchcraft, we do not wish to ignore other dimensions—ego functions for the individual, projection via scapegoating, etc.—as partial explanations of these phenomena. Whatever adaptive functions magic, witchcraft, and sorcery may have for the individual, there is little doubt that they are linked to the social order in any of several ways. As Kluckhohn points out, they contribute to societal continuity by defining what is bad, namely, all secret and malevolent activities against the health, prosperity and lives of fellow men (Kluckhohn, 1944, p. 30). Witchcraft may be used to symbolize group affiliations by being turned against outsiders. It can be used by leaders to bolster

ways. If one knows that he has an important examination coming and he also knows that he has eaten eggs for breakfast, these sets of knowledges are *irrelevant* to each other. If one knows he has an important examination and also knows that he is busily engaged in studying for it, these knowledges are *consonant* with each other; the cognition of "studying" follows from the cognition of "an examination." If one knows he has an important examination and also knows that he has social obligations which will fill his free time before the examination, then the knowledge of the coming test and the knowledge of the dates which will preëmpt his time are *dissonant* with each other: keeping the dates does not follow from the cognitions about the examination: studying for the examination does not follow from the cognitions that one is expected elsewhere.

When two cognitions or sets of knowledges are dissonant with each other, there will be set up in a person tensions of psychological discomfort which motivate the person to try to eliminate or reduce these tensions. "Dissonance" is the term applied to these tensions which arise when cognitive elements do not follow from, are dissonant with, each other. To reduce dissonance, one must modify the cognitive relations which produced it—that is, one must attempt to make his cognitions consonant with each other. As formulated by Festinger, these are the postulates of dissonance theory:

(1) When dissonance is present, in addition to attempting to reduce it, one will actively avoid exposure to situations and information which might increase it.

(2) The magnitude of dissonance increases as the importance of the cognitions on which it is based increases, and conversely.

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their authority (Hunter, 1936, p. 317; cf. the role of the king in Swazi society as reported by Kulper, 1947). Witchcraft is often directed toward certain categories of deviants, while underlying beliefs affirm and reinforce behavioral norms (see Hogbin, 1934, pp. 149-152; also see B. B. Whiting, 1950, pp. 48-55 and Kluckhohn, 1944). The vast majority of those whom Kluckhohn heard accused of witchcraft were persons of wealth and prestige. Behavior which leads to the accumulation of wealth and power is proscribed, because it runs counter to Navaho values: hospitality, generosity, and prodigality in sponsored ceremonials. "Accusation of witchcraft is, indeed, a threat which Navaho social organization uses to keep all agitators (i.e. deviants), all individuals who threaten to disrupt the smooth functioning of the community, in check . . ." (*idem*, p. 164). In addition, witchcraft may be correlated with certain age and sex categories, and thereby reflect strains in the role system (cf. Evans-Pritchard, 1937, p. 31, and Hunter, 1936, p. 307). For a discussion of this point in greater detail, see Nadel, 1952.

7 It is not supposed that the material presented will constitute a *proof* of this proposition. Our intent is to advance a coherent set of tentative hypotheses, supplemented by illustration, which may serve as a stimulus to further investigation. At times we cite negative data; i.e. that a given practice *does not* exist in a culture. We are fully aware that such assertions are very difficult to prove. It may be simply that the field-worker overlooked—or did not think it worthwhile to report—a given practice and, at least to this extent, one must exercise caution in evaluating our illustrative material.

(3) The total amount of dissonance present between two cognitive clusters is a function of the proportion of *elements* of each cluster which do not follow from each other, the elements being weighted in terms of their importance to the individual.

(4) When dissonance exists between a knowledge about the environment, or a belief, and an aspect of one's behavior, it is usually easier to change one's behavior to reduce dissonance, than it is to change the environment. But if a behavior is very important to one, it may be extremely hard to change. A smoker who thinks cigarettes harm his health but who cannot stop smoking may (a) change his belief that cigarettes are harmful or (b) add cognitive elements consonant with smoking, e.g. that cigarettes keep his weight down. Either a or b or both, will reduce the total dissonance. If the man had been able to abandon smoking, that would also have reduced the dissonance.

(5) Another way of reducing any dissonance is by seeking social support; if the man's friends were to agree with him that cigarettes are harmless, their agreement would provide cognitions consonant with continued smoking.

(6) Following a *decision*, then, active seeking out of information and opinion consonant with the action taken is expected. For reasons inherent in Postulates 1-5, the successful reduction of post-decision dissonance makes a reversal of the decision more difficult.

(7) The maximum dissonance which can exist between two cognitions equals the resistance to change of the less resistant element; if dissonance reaches a point where it exceeds that resistance to change, the cognition will be changed, thus reducing the dissonance.

(8) Individuals are differentially tolerant to dissonance; dissonance sufficient to induce change in person A may not be very troubling to person B.

This does not pretend to be more than a brief summary of dissonance theory: additional aspects will be introduced as we proceed.[8]

We have implied that supernatural practices serve, at least, two different functions, handling problems in the environment which are not subject to empirical control, and handling pressures which may arise in the individual, such as anxiety and frustration, when his environment deprives or threatens him. We shall call the first, or tool-like usage, the *coping function*, and the second usage, which attempts to alleviate internal distress, the *compensatory function*. We shall next attempt to illustrate and expand upon these notions, in order to cast the data on magic into a frame of reference which will later enable us to treat the relationship between magic and dissonance-reduction.

8 The complete formulation is available in Festinger, 1957.

It seems reasonable to assume on the basis of available experimental evidence, that when an individual desires to modify his environment, ordinarily his first impulse will be toward direct action. If this is unsuccessful, he will attempt indirect methods. For example, when a child in a playpen with a stick sees a toy outside the pen, there is first an attempt to climb over the pen, or reach an arm through the bars. If the toy is beyond reach, the child eventually learns to utilize the stick in manipulating the desired object. Or he may first attempt to induce the observer to bring the object closer.[9] In either case, the child tries to use the stick and the person as tools; such indirect action, if successful, will achieve the same end as would have been managed by more direct means.

II

In part, we have conceptualized magic as a sort of tool, a control device which substitutes for empirical means of dealing with environmental problems. If this is the case, not only should the practice of magic vary inversely with the availability of empirical techniques (see notes 2 and 4), but should reflect changes in the status of the problems for which magic is employed. Also, if magic is a tool, this implies that it will be related specifically to some environmental complex of phenomena—so that we should not expect the wholesale introduction, or abandoning, of a generic "magic."

When a tool is ineffective, one seeks a better tool. It follows that, with no change in empirical technology, if magic is somehow removed, rejected, or disproved, we should expect the development or acquisition of alternate forms of magic. The Azande, not satisfied with their own witchdoctors, have gone abroad to find better magic. At the time Evans-Pritchard was in residence they were importing new techniques and "medicines" from their eastern neighbors.[10] The Azande are practical about their magic, sometimes using several methods to achieve the same purpose, e.g., in keeping rain away.

Azande husbands are much concerned about their wives' fidelity, yet adultery is prevalent. At the time of the report, there had been imported from the Belgian Congo a new magical specific intended to produce venereal chancre on anyone who might copulate with the user's woman. Other new magic items for hunting and property guarding have been imported to replace the ineffective local varieties.[11] Azande princes and courtiers use magic far less than provincials. Men at court—who engage in fewer practices for which magic can be employed—are often contemptuous of much of the magic which aids the provincials in hunting, collecting, and agriculture.[12]

9 Albert, 1949.

10 Evans-Pritchard, 1937, p. 201.

11 *Idem*, pp. 406, 409, 481.

12 *Idem*, p. 425.

When the job for which one uses a specific tool is no longer performed, that tool is no longer employed. It follows that where an activity is abandoned, we should expect decline or disappearance of the magic appropriate to the activity. In Malaita, as the result of government suppression of warfare, spells for success in battle have been abandoned, but medical facilities are still poor and the same importance attaches to magic for health and illness.[13] Among the Yaqui Indians, where White contact has reduced dependence on the natural environment, the animal gods are becoming mere entertainers.[14]

If a totally new task arises, one seeks new tools or adapts old tools for the task. From this it follows that, holding technology constant, where there develops a new and ongoing environmental phenomenon which exerts the concern of the culture, we should expect magic to be born or acquired. There is considerable evidence to support this proposition; part will be presented here, and part is cited throughout the remainder of this paper, where relevant to additional considerations.

Navaho witchdoctors have often tried, unsuccessfully, to make new magical songs for the White man's tuberculosis, measles, influenza, and syphilis. Recently smallpox has become a divinity of the Ho in India.[15] Among the Pondo, while magic to escape danger in battle is no longer useful, it is thought to aid in escaping danger from the police.[16]

The Azande, during the first twenty years of this century, adopted several associations for the practices of magic. In so far as their rites do not replicate magical practices already existing, they are directed against the situation of European rule.[17] Hogbin points out that in Busama, prayer is used as if it were magic, and is sometimes employed to cope with problems for which no precedent exists.[18] Wilson, who disagrees with Dr A. Richard's contentions that the new complexities of African life have resulted in increased magic, nevertheless notes that Christianity is treated quasi-magically by many natives.[19] In accounting for the appeal of the Peyote cult, Burlin points to the belief that its followers would be cured of alcoholism and tuberculosis—diseases which had been introduced by the Whites.[20]

Also, there is growing evidence to support the hypothesis that where direct control of the environment is adequate, there is little magic.[21] For example, the Azande courtiers, whose duties entail few chores of an agricultural, hunting, or food-gathering nature are rather scornful of much "bush" magic, which relates to these activities. Evans-Pritchard writes:

13 Hogbin, 1939, p. 174.
14 Spicer, 1940, p. 136.
15 Webster, 1948, pp. 357-358.
16 Hunter, 1936, p. 495.
17 Evans-Pritchard, 1937, p. 201.
18 Hogbin, 1951, pp. 272-273.
19 Wilson, 1954, p. 120.
20 Burlin, 1956, p. 113.
21 Firth, 1939, chap. V.

> The principal old established medicines are used by most commoners of high standing at court, and even by princes themselves, for attracting dependents . . . avenging theft . . . [and] medicine for homicide has gained in popularity since direct vengeance has been prohibited by European administrations.[22]

Or again, among the natives of Malaita, retaliation for most crimes or injuries inflicted by non-villagers is via magic. An exception has been murder, the most serious crime. Here the usual method was direct action by raids on the offender, and these raids often brought counter raids. Therefore, in the past only the deaths of important men were avenged by raid. But at the end of the 19th century, when the introduction of firearms facilitated murder, raiding increased sharply.[23] These examples appear to illustrate that (a) where direct action is readily possible it will be preferred to magical means, and (b) where direct action is not readily possible, magical means may be used to substitute for it. However, it is not implied that magic is the only form of indirect action available, or likely to be employed.[24] We have considered the example of the child in his playpen, attempting to gain possession of a toy, first by direct and then by indirect means. If, for some reason, the child is unable to attain the toy, he may withdraw from the psychological field by abandoning the project. In such case, very likely he will console himself in some way: he may throw himself into play with available toys; he may devalue the merits of the unattained toy. Such behaviors are compensatory.

Where frustration involves more important matters, persons may develop compensation via fantasy or aberration. In an extreme case, a wish-fulfillment fantasy may become prolonged into a delusion. We note that in a situation of frustration,

22 Evans-Pritchard, 1937, p. 426.

23 Hogbin, 1939, pp. 82-91.

24 In Siuai, for example, while sorcery is prevalent, the social pressures to pay debts are effective without any magic (Oliver, 1949, p. 15). Often it becomes impossible to extricate purely magical action from non-magical action. For example, magical and political means of handling inter-group conflict may be inseparable. The Elema natives are in many ways reminiscent of the Busama: in both groups, sorcery is related to control by the high-status elders and results in a very conservative pattern; in both places there is a correlation between increasing rank and increasing likelihood of sorcery. The eldest men of Elema form a semi-formal council, the *avai*, which enjoys special prerogatives. Presumptuous would-be members are liable to punishment by sorcery —for if the *avai* are not all sorcerers, many are, and their influential connections permit them, as a body, to set sorcery in motion. In their ceremonial functions, the old men are empowered to enforce propriety by their sorcery powers. In one instance, an inter-village battle developed as the result of murder in revenge for supposed sorcery. The battle had continued some days with a number of casualties, when one of the age-classes thought of calling the *avai* to perform a ceremony which requires internal harmony when held—as a sign of reverence. The *avai* came and left a number of broken weapons on the beach at night to indicate that the conflict must cease. Both sides were ready to obey. Perhaps they were eager to end the battle, but it is said the belligerents feared non-compliance lest they be punished by the old men's sorcery (see Williams, 1940).

individual differences exist: an instance sufficient to produce aberration in a given man may not affect other men who are in the same objective position so drastically.[25] We ordinarily describe such variations as differences in stability of the personality.

We have seen an example of withdrawal from the field by the Navaho medicine men who have given up trying to cure "White man's diseases" magically. It is a frequent rationalization of some native witchdoctors that their medicine will work on fellow-tribesmen, but is ineffective against Whites for whom it is not an appropriate tool.

Where a person or group is placed in an ongoing, frustrating situation as the result of domination by an out-group (or a single person), one way of compensating is to adopt the values of the dominating person (s). Bettelheim[26] tells of value changes in Nazi concentration camps among the prisoners. In such relationships there is considerable ambivalence, and to the extent that a group feels both "A" and "not-A" toward another group, we may conjecture that actions consonant with "A" are dissonant with "not-A" and conversely. It should therefore not be surprising if the behaviors of a subordinate group in such a situation would appear, to an outside observer, "over-reactions" by a criterion of common sense.[27]

We have suggested above (p. 144) that magic may serve not only as an alternate to empirical techniques, but may also serve in handling the anxiety, frustration, or threat which may result when people are confronted by important environmental forces not adequately handled by their empirical social or political controls. To the extent that supernatural practices are employed in this latter manner, they illustrate the *compensatory function.*

Let us look for compensatory practices in the data on magic. In Navaho culture, witchcraft provides scapegoats for the release of hostility, and one may attack enemies by whispering that they are witches.[28] It would thus be expected that the need for a scapegoat would vary in the same sense with the degree of stress, anxiety, and hostility induced by environmental circumstances. There is agreement that the period (1875-1890) after Fort Sumner saw a maximum of witchcraft suspicion,

25 Cf. Kluckhohn, 1944, p. 62.

26 Bettelheim, 1957.

27 Cf. Kluckhohn, 1944, p. 62. Perhaps the clearest illustration of this phenomenon is seen in the parent-child relationship: the child is said to internalize the values of his parents and to develop attitudes of ambivalence toward them. Characteristically, the child feels strong love at a given moment, and intense hostility at another; his attitude swings to the extremes of the polarity; he does not experience an "averaging" of positive and negative effect.

28 *Idem*, p. 66.

witch trials, and possible withcraft practice.[29] This was a period when the equilibrium of Navaho society, destroyed by war and captivity, was in a troubled state of readjustment.[30] "A correlation between the amount of fear and talk about witches and the general state of tension prevailing among the Navaho is evidenced by the fact that during the recent difficult years of controversy over the [live-] stock reduction program there has been appreciably more witchcraft excitement than for some time past. . . . There have also been several well-documented attempts by Navahos to 'witch' government employees concerned with stock reduction."[31] It may be inferred that the hostilities and anxieties which were released within the group had been diverted from the Whites who were the actual aggressors, to the 'witches' who were mere scapegoats. Hogbin maintains, in reference to another compensatory function of magical ceremonies, that their purpose is to make the future secure, and that the result of the magical practices and ceremonies is increased confidence. Because life in a community which possesses little adequate empirical knowledge is uncertain, the effect on morale induced by performance of magic is quite important.[32] Virtually every observer who attempts interpretation for the data examined, makes similar points.[33]

The Taro cult among the Orokaiva, which has developed since the start of White domination, has brought no new forms of coping magic, although it has introduced new forms of ritual for preëxisting usages. The significant innovation is the *jipari* seizure, a kind of fit similar to that of the West Coast Shakers. Note that the Taro cult arose after severe government suppression of a similar Baigona cult, which latter had introduced a kind of mass paroxysm similar to, but much more violent than, *jipari*.[34]

Unlike the Ghost Dance cults which preceded it (and were contemporaneous until government suppression), the Peyote cult introduced no new coping magic hostile to the Whites, or intended to reëstablish native supremacy. Its signal feature is the use of the mescal, ceremonially, to produce visions, trances, and the like. Slotkin reports:

> The Peyote Religion was nativistic but not militant. . . . It provided a supernatural means of accommodation to the existing domination-subordination relation,

29 Using a somewhat different terminology, we might say that, in a situation of *frustration*, the Navaho employ witch excitement as a kind of *aggressive* response. Cf. Dollard *et al.*, 1939, pp. 39-54 esp.

30 Kluckhohn, 1944, p. 64.

31 Kluckhohn and Leighton, 1947, pp. 175-176.

32 Hogbin, 1939, pp. 109-110.

33 Cf. Evans-Pritchard, 1937, p. 253 and Kluckhohn, 1944, p. 56.

34 Williams, 1928, pp. 8-9.

[which] as opposed to the Ghost Dance's program of opposition, was the basic reason for the former's success and the latter's failure. . . . The [Peyote] adherent could resign himself to the present objective subordinate status for three reasons: (1) The ethic minimized the importance of this subordination, so that one could obtain a subjective tranquility. (2) The rite provided a technique of communion with the spirits; this temporarily lifts the adherent from the commonplace level of mundane existence to the exalted level of spiritual sublimity. (3) According to the religious eschatology, those who conform to the religion's ethic in this world will be rewarded with eternal bliss in the next world, in the company of spirits and loved ones.[35]

We note that the eschatology is similar to Christian, and the present form of Peyotism considers itself a Christian church. Here, then, is an instance of the adoption of the values (or forms) of the dominant out-group, as well as compensatory supernatural behavior.[36] Linton[37] makes somewhat similar points, and draws somewhat different conclusions.

Items indicative of the adoption of dominant out-groups' values are frequently present in newly-developed militant cults as well; militant in that the magic and/or ethos are aimed at coping directly with the dominators. For example, Williams cites a letter from a missionary describing the birth of a short-lived cult. There was a prophecy of a flood which would destroy the hated Whites and all natives who refused to become followers. He also observes that the faithful were to be resurrected in white-skinned bodies.[38]

We have suggested that compensatory magic would likely arise in a situation of prolonged stress, frustration, or anxiety. Also, it has been noted that individuals differ in their ability to withstand such stressful situations. It follows, then, that insofar as magic is a compensatory means of handling anxiety and the like, the individuals least well-equipped to withstand stress ought to be among the first persons to promote the use of magic.

The source for the inspiration behind a cult in Manau was the dreams of its founder, Buinina, who as a boy was subject to seizures.[39] Taro cult subsects have been formed as the results of the respective founders' dreams during a time of mourning the very recent deaths of near-relatives. The Ghost Dance of 1890 arose as far as Basin and Plains tribes were involved from the visions of Wovoka, who believed himself a prophet with a divine revelation to impart. The Ghost Dance was spread by an epidemic of dreamers among the natives, although after a while

35 Slotkin, 1956, pp. 21, 40.

36 Insofar as a cult becomes institutionalized through time, it takes on the character of religion. But in its time of origin and early development we shall treat a cult as a form of magical activity.

37 Linton, 1943.

38 Williams, 1928, pp. 100-101.

39 *Idem*, p. 82.

dreaming began to concentrate in the hands of a few specialists.[40] The recent Guardians cults in the United States began as the result of its founder's "messages."[41] It thus seems probable that a good part of such movements derive from the less stable individuals in the given group. Barnett[42] and Williams[43] draw similar conclusions. Williams suggests that not all such visions are sincere and may be prompted by ambition, jealousy, and the desire for notoriety.[44] In at least one instance, a new cult apparently developed from conscious profit motives of its founder. This was the flood cult on Santo Island whose tenets provided for the death of all non-members in the coming deluge. The chief "prophet" charged fees in pigs and money as a condition of admittance.[45] Williams also makes the significant observation that where a vision induces trances, epileptic fits, or other visible aberrations these abnormalities help to convince the natives that the visionary is endowed with special power.[46]

Compensatory practices provide a method of adjustment to the duress of outgroup dominance. It seems unlikely that such an approach, based on resignation, would be wholly satisfactory to the native. More probably, at least during the earlier stages of conquest, some attempts would be made to replace compensation by more direct means of coping with the environment, in this case, militant action or hostile magic. Naturally, this is a difficult area for investigation by a White ethnologist. Of the Taro cult, Williams says that the members preserve secrecy, but that there is good reason to suppose that they practice sorcery; no specification of content is made.[47] Similarly, Evans-Pritchard cautions that his material on the Azande magic associations is incomplete.[48] It may be that some of the practices of the five other groups were more direct in intent than those of the Mani association.

However, there is one clear source of evidence. Among Indian tribes in the United States, there have been formed several militant political action groups, intended to overthrow the domination-subordination relationship pattern. In addition, Slotkin reports:

> Other militant action groups are supernaturalistic in character. There have been a whole succession of minor religious movements which have diffused rapidly from one reservation to another. The following are two characteristic religious phenomena which spread across the north central states during the 1940's and were kept from Whites:

40 Lesser, 1933, pp. 96-101.
41 Festinger, Riecken, and Schacter, 1956.
42 Barnett, 1941, pp. 166-167.
43 Williams, 1928, pp. 77-83.
44 *Idem*, p. 78; cf. Kluckhohn, 1944, p. 49.
45 Williams, 1928, pp. 100-101.
46 *Idem*, pp. 82-83.
47 *Idem*, p. 64.
48 Evans-Pritchard, 1937, pp. 512-513.

(a) Jesus is to be reborn again as an Indian savior, and every now and then the rumor would spread that a child on some reservation was the reborn Christ. (b) A big wind will sweep across the country, destroying all Whites and any Indians who follow White customs; only Indians conforming to traditional cultures would be saved."[49]

III

Let us make the assumption that the process in which a person is introduced to, and adopts, an item of magic is equivalent to making a decision. Magic, as it has been conceptualized in this paper, is an implement. Its owner can use or abandon it, like any other possession. It has been developed above that magic comes into being as a symbolic alternate method of controlling the environment. We may infer that relative to the importance of the aspect of the environment concerned, possessing some means of control is important to the individual and to the group. Thus, in some areas, it will be very difficult to surrender a belief in the efficacy of magic, presuming technology is held constant. This is especially true where magic represents an externalization of deep, acute anxieties. From this it follows that: to the extent that the item of magic is seen to be ineffective, dissonance will be generated by the continued use of the item. Where an item of magic is in use within a group, throughout that group there will be dissonance in using the item, unless the magic is effective. The more important the magic-item, the greater will be the dissonance.

Most writers have seemed surprised by the tenacity of magical practices among primitive groups. Webster in his encyclopedic study, for example, offers references to tribes where magic persists, in Australia, Torres Straits, Dobu, Solomon Islands, Fiji, Africa, and British Columbia—where the Bella Coola have not relinquished their belief in magic after a century of contact with Whites.[50] He cannot account for this, however.

Such resistance to change would appear to be a direct derivation from dissonance theory. Let us first take a hypothetical example, and then attempt a more formal statement. Let us suppose an aborigine who lived all his life among neighbors and kin in a tribe that holds a belief in a given item of magic. Let that item be relevant to an area of action which is extremely important in the life of the tribe. It is doubtful that the item will always be thought to achieve its purposes. Even with eloquent rationalization by the tribe, we would have to posit the introduction of dissonance between knowledge of the magic failures and knowledge that the magic continues in use. Here is a perfect situation in which social support may operate to reduce dissonance. There would doubtless be some discussion of the matter. Let all tribesmen agree that the item is excellent magic. Thus, all the persons known to the aborigine hold the same belief. He and they use the magic

49 Slotkin, 1956, p. 49.

50 Webster, 1948, pp. 504-505.

item. Suppose now that a missionary comes and attempts to discredit the efficacy of the magic. Dissonance is increased; the belief is important; social support is present, and the item comes to be maintained more firmly than before.

The Azande reflect the outcome of such a hypothetical historical sequence in their attitudes toward and use of certain kinds of magic. Thus they make moral and social demands on kinsmen to avenge the death of a relative, but the government imprisons those who practice vengeance on others. Magical retaliation is the solution, and is used by nearly everyone, its owners charging heavy rental fees. Doubts about its efficacy would be extremely dissonant with the importance and prevalence of the magical item. Vengeance magic enjoys the highest prestige.[51] While the Azande admit the possibility that a magic rite may fail, they are most often sure that vengeance magic will be successful.[52]

One other item of magical practice is of extreme importance to the Azande. This is the poison oracle, which, administered to fowl, is the ultimate method for determining truth in all important matters: it has the force of law when employed on the orders of a prince; virtually no important venture is attempted without consulting the poison oracle. The Azande believe the poison oracle is infallible.[53] Yet, the author states elsewhere that, if not backed by the king, the poison oracle might have been abandoned. That is, we may infer dissonance and subsequent increase of belief with continued use of the oracle. Note also that in most magical practices, the Azande tend toward skepticism: the lesser oracles have little importance and many people ignore them.[54] The efficacy of anti-theft magic, for example, was generally doubted.[55] For most magic, which is of less importance and therefore less resistant to change, there is considerable qualification, doubt, and among some individuals, outright disbelief. Perhaps it should be added that before the government suppressed retaliation by violence, the Azande had no such utter dependence on vengeance magic as now exists; as with the poison oracle, enforced use and the press for dissonance-reduction have combined to make the belief in vengeance-magic extremely tenacious.

Nevertheless, it is beyond dispute that given enough time and persuasion, the resistance to change may be overcome.[56] Also, where a new technique stands in

51 Evans-Pritchard, 1937, pp. 424-428.

52 *Idem*, p. 466.

53 *Idem*, pp. 260-262.

54 *Idem*, p. 378.

55 *Idem*, p. 460-467.

56 There are many cases of such eventual acceptance of a practice which at first was firmly resisted. For example, among villagers of Hyderabad, Dube reports that the opening of a small modern medical dispensary has considerably changed the attitude of the people towards disease

an irrelevant relation to preëxisting cognitive elements of a person or group, there should be no such firm resistance to the technique. It is thus reasonable to expect a given tribe to resist some innovations and to accept others readily.

One reason which makes it difficult to identify the presence of dissonance and attempts to reduce it, except for the rare example, is that the total cognitive framework of a given native group will often be based on assumptions very different from our own. A set of relations among knowledges that would be dissonant to the Westerner, may be irrelevant to the tribesman, and conversely. Analogous statements are made by many observers about native rationales. If one grants the implicit assumptions, certain forms of magic become self-validating.[57] Belief in counter magic or sorcery interfering with given magical cures also tends to reduce dissonance.

Where the use of a practice is dissonant with items of knowledge which discredit the practice, the dissonance may be reduced by abandoning the practice or by adding cognitions consonant with its continued use. Illustrative of this is skepticism about the powers of witchdoctors in the Nicobar Islands, some of whom, despite many rewards, refuse to practice.[58] This would appear straightforward, and as expected. One could assert that, without reinforcing their degree of belief, witchdoctors could continue to practice for the rewards alone. But the data further establish that when a witchdoctor takes ill, he will often call another witchdoctor to treat him; other witchdoctors may treat themselves or sick members of their families.[59] Such actions are not obvious without inferring strong belief, and Webster cites extensive references to these practices.[60]

Where there is considerable fear and anxiety about a practice, there will be dissonance between the presence of the fear and/or anxiety and doubts about the validity of the practice. We would expect that sometimes the belief in the practice will be heightened, even though the existence of the practice may appear detrimental to the interests of the group holding the belief. This is essentially like the

and its treatment. Where before the people ran away from the Government vaccinator, or offered bribes or threats to be spared the hypodermic, there is now increasing use made of modern medicine. But there is still recourse to magical healing as well (Dube, 1955, p. 221).

57 For example, Hogbin ascribes the natives' belief in their magic to their poor health; for many diseases there is no known cure except magic. Thus, since the average Malaitan spends two to three weeks each year bedridden, every performance of sorcery is seen to be followed by the "victim" becoming ill. Once two men confessed they were producing the illness of a third man—whom Hogbin was trying to cure! (Hogbin, 1939, p. 87). Where any dissonance is present, such instances are probably transmitted widely as confirming the beliefs. (Cf. Kluckhohn and Leighton, 1947, p. 172).

58 Webster, 1948, p. 475.

59 *Idem*, p. 477.

60 *Idem*, p. 491.

anxiety-justifying rumors reported by Prasad[61] and a similar observation made by Kluckhohn.[62]

In this connection we are also confronted with the observation that belief in witchcraft and sorcery, and fear of these rites, tend to be associated with the belief that local sorcery is weaker than that of other groups. The Navaho often employ Pueblo Indians—very much feared as witches—to cure witchcraft;[63] the Alur make similar use of persons from tribes considered inferior.[64] The natives of the Murray Islands believe foreign sorcery to be deadly at a range of one hundred miles. The Kiwai Papuans think their neighbors are potent sorcerers. Beach and mountain Arapesh attribute all misfortune to the sorcery of the plains Arapesh; the beach and mountain dwellers own no magic. The Trobriand Islanders consider the most potent of all sorcerers to be the nearby Dobuans.[65] From a common-sense viewpoint, these data would seem as probable as Lord and Taylors singing the praises of Nieman-Marcus, or Germans extolling the superiority of British weapons.

This apparent paradox can be resolved by reference to the proposition that where a practice is important to a group, and the ineffectiveness of that practice leads to dissonance, often this dissonance will be reduced by attempting to change the social environment. This can be done (as with the smoker who did not give up cigarettes, but got his friends to agree that smoking was harmless) by getting agreement about the validity of the magical belief from group members. If all the group shares similar dissonance and if any member should dissent, we should expect that he will be subject to strong pressures to conform; if he persists in dissenting, he may be rejected by the group. Dissonance can also be reduced by co-opting members of neighboring groups to the belief and thus reinforcing it. Where a group *fears* sorcery greatly, but actually *practices* it only slightly, there should be dissonance between the degree of fear and the scarcity, or inadequacy, of the sorcery. Hence the attributing of more effective sorcery to outsiders. The pressure to reduce dissonance will be a function of the importance of the belief (s) which stand in dissonant relations with environmental facts—here, the failures of the magic. The result should be increased belief in the item (s); the more important the item (s), the more firmly the belief (s) will come to be held.

We note that this theoretical derivation opposes the view that, necessarily, by educating selected members of a "primitive" group and sending these "enlightened" natives back to their people, the result will be transmission of enlighten-

61 Prasad, 1957.
62 Kluckhohn, 1944, p. 49.
63 *Idem*, p. 35.
64 Southall, 1953.
65 Webster, 1948, pp. 373-374.

ment to the natives. Rather, it is derived that acceptance of, or resistance to, the innovations depends in large part on the manner and degree to which the innovations are consonant or dissonant with the natives' prior beliefs.

Adair, in this connection,[66] reports a study on the nature of the adjustments to civilian life, by returning Zuñi veterans of World War II. Because of its special relevance to the present formulations, we feel obliged to quote extensively from it:

> The pueblo of Zuni went through a period of great tension during the years of World War II because the impact from the outside world was greatly intensified. . . . There is good evidence of the fact that the Zuni are more occupied with witchery now than they have been at some periods in the past. With the increase in anxiety due to uncertainties in the minds of the villagers and conflicting values in the minds of the men returning home, there was an accompanying increase in witch beliefs, and this in its turn acted as one of the controls which served to subjugate the individual to the will of the community.
>
> Thus, many of the veterans have not utilized the knowledge, or followed the ideas and habits picked up while they were in the service. . . . While the men were away from the villages, religion was an individual matter; as soon as they returned, they were forced to conform with traditional ritual practices enforced by the public opinion of the pueblo. There was a very definite "bid" for the men on the part of the curing societies, but it is not known whether this process is a carry-over from the old war practices of the past, or a new development based on a conscious desire of the leaders of the various curing societies to recruit new members. There is a slackening of activity in the katchina cult on the part of the young men, but this probably represents a general trend rather than a function of acculturation among veterans. Lack of faith in the katchina cult and the ritual procedure of the rain priests is in striking contrast to the firm belief of these men in the efficacy of treatment offered the sick by the curers. Belief in the weather control has been undermined by the changing economy; since there is an alternative way of obtaining food, that is, working for wages, the motivation for dancing to obtain rain for the crops has shifted from one of necessity to one of social conformance. Medical treatment offered by the government has not become an alternative method of curing in the sense that wage earning has become an alternative method of obtaining food, because of the necessity of dealing with witches, as long as anxieties are expressed in that way.
>
> The following techniques of enforcing public opinion are discussed: gossip, rumor, ridicule, and formal action taken by the pueblo council. Ridicule, rumor and gossip—because of the strong sense of shame—are especially effective devices. These three (other than formal action) form a kind of superstructure which rests on the underlying sense of shame, and they are as important as witchcraft in determining "social timidities" (Parsons) and in shaping the Apollonian (Benedict); as a result of the sense of shame, a Zuni feels uncomfortable whenever he is made to feel conspicuous. All aspects of behavior which deviate from the norm become the subject of village gossip; two "subjects" of gossip are presented; (1) the feeling of the Zuni towards

66 Adair, 1955, pp. 19-24.

those of their fellows who aspire to "act like whites" and (2) gossip leveled against those who deviate from the religious patterns. It is quite possible that the gossip of the first sort proved to be a greater block to these men, as carriers of new traits and ideas brought back from the outside, than any other cultural technique. It forced many of these men to change their ways shortly after their return: if they persisted in their ways they were made to feel uncomfortable. This gossip is so powerful that the veterans have not been effective as culture carriers.

Conclusions . . . 4. Because of the period of stress created by the war the anxieties of the people have increased. Witchcraft is, at Zuni, a traditional method of displacing these anxieties, that is, attributing one's personal difficulties not to individual failure, but to conceptualized scapegoat: the witch.

5. The 213 Zuni men who served in the armed forces learned new habits, ideas and techniques, and acquired new attitudes and values while they were away from the pueblo. Native belief in witchcraft and religious activities have acted as controlling forces and have drawn the men back into their native way of life. Public opinion in the pueblo as it is expressed in the form of gossip, rumor, and ridicule has been a most effective technique in the enforcing of the traditional way of life and has acted as a deterrent to these men. As a result of these barriers no non-pueblo traits of significance had been introduced into the pueblo at the time of the research, (1948). . . .

8. An equally significant point is the "drifting-out" process that is a most important dynamic in modern pueblo culture, and has been of great importance in the retention of the old ways. There is a steady drift from the pueblo of those who are not willing to conform to the traditional ways of the Zuni. The result of this is that those who might be cultural innovators are lost from the stream of Zuni life. A selective process is at play: it is the non-conformist who leaves the village.

9. Zuni is a much less friendly pueblo, insofar as its relations with whites are concerned, than it was 30 years ago. It is probable that this attitude towards whites has diffused from the Rio Grande pueblos, which have been hostile to whites for a long time. Growing hostility towards whites is certainly in part a result of the encroachment of the white world on what has until quite recently been an isolated pueblo. The Zuni are acutely aware of the fact that certain aspects of their ritual are being lost. It is basic to Zuni belief that to share a religious secret is to destroy its efficiency. By the same token, becoming more secretive may increase the power which has gotten out of hand. Is it a successful technique of handling the same situation (as at the Keresan and Tanoan pueblos)—secrecy and hostility toward whites? This recent antagonism toward whites is another reason the white ways carried back to the pueblo by the veterans have not taken root.

10. Accompanying the hostility toward whites is a certain "nativism" that may be seen in the revival of ceremonies at least three of which have been given for the first time in many years. It looks as if, in the face of certain aspects of disintegration of the total religious structure, other parts are flourishing, at least for the moment.

We have felt it necessary to quote the Adair study at some length, in part because it provides illustrative continuity for the theory developed above; in part,

to allow basis for differing interpretation of all the data. We have already noted how magic may function in social control.[67] But the significant facts for us are these:

1. The katchina cult and rainmaking procedures had, in the pueblo, diminished in importance as beliefs. This stems from the availability of an alternate, and more direct, means of obtaining food: via wage work.

2. The curing societies had not diminished in importance but had *gained* in *belief* by the pueblo. This follows from (a) their function in curing witchcraft, for which they are the only available means, and (b) the fact that anxieties attending further encroachment of the Whites had increased concern with witchcraft.

3. Accepting and participating in both the curing societies, and the katchina cult and rainmaking ceremonies, are group norms. That is, to the extent that pressures toward conformity *other than dissonance-reduction* may be at work, these pressures should affect the katchina cult, rainmaking ceremonies, and the curing societies equally. *Differences* in pueblo demands for conformity in regard to these three sets of practices cannot be ascribed only to pressure to conform to traditional norms.

4. There was a slackening of activity in the katchina cult and the rainmaking ceremonies on the part of the returned veterans; this, despite the fact that conforming in these practices is a traditional Zuñi norm.

5. The curing societies made definite efforts to recruit new members from among the returning veterans. We infer that (a) the importance of the curing societies as the only means of coping with witchcraft would be dissonant with any cognitions that the curing societies were ineffective; (b) if the returning veterans failed to accept the validity of the curing societies, this would augment dissonance for the other members of the pueblo; (c) if the returning veterans accepted and participated in the curing societies, this would provide social support consonant with the beliefs of the other members of the pueblo, that the curing societies were effective.

6. There was firm belief on the part of the veterans in regard to the curing societies. This is in direct contrast to lack of faith in the katchina cult and rainmaking ceremonies. We infer this difference to be the result of greater pressure to conform in, and more active proselytizing in regard to, the curing societies on the part of the pueblo. These acts, in turn, are indicative of pressures to reduce dissonance made by the pueblo, in line with the derivations.

67 More explicitly, social control is one segment of environmental control. We have defined *witchcraft* and *sorcery* as those aspects of magic which are primarily employed to control the social environment. Cf. Nadel, 1952, pp. 28-29.

7. There is a steady drift from the pueblo of those who are not willing to conform to the traditional practices of the Zuñi; it is the non-conformist who leaves the village. We infer this exodus on the part of the deviant members to indicate that, where the pueblo seeks to exert pressure to conform on its members, the pressure is adequate to obtain conformity in most cases, and adequate to make rigid deviates uncomfortable enough so that they leave the group. We note that, by failing to subscribe to pueblo beliefs, (a) the deviants would be exactly the persons who would increase dissonance for the pueblo, and (b) the deviants would therefore be most subject to pressures to conform; if ignored, these pressures in the form of public opinion, gossip, and the like might be expected to make life hard for the deviants. Consequently, this study appears to offer reasonably unambiguous support for the derivations.

It has been demonstrated elsewhere that, like the Zuñi curing societies, when members of a group possess identical dissonance, they may attempt to reduce this dissonance by proselytizing.[68] In effect, this is the same process an individual uses when he attempts to reduce dissonance by seeking agreement from his friends. We have tried to show that magic, because relatively ineffective, will often introduce dissonance into the group which practices it. The cognitive relations which give rise to this dissonance will be very similar for all group members.

In the case of the Guardians Cult, it was possible to proselytize among the numerous members of the community-at-large. That is, ignoring individual susceptibility, the cult was surrounded by a population whose members, in common with the cult's, were 20th century United States citizens. To some extent, the community-at-large offered a better chance of providing converts than would a population of 18th century orientals.

To continue the argument, let us imagine a tribe which has recently adopted a supernaturalist cult. The cult is essentially a complex of magical items—some may be of coping magic, some may be of compensatory magic, in many combinations of kind and ratio. We have tried to show that members of the cult may suffer from identical dissonance. In part, this may be reduced by obtaining converts to the limit of the population of the tribe. But even if all tribesmen are now members, the dissonance may not be sufficiently reduced for comfort. Additional social support may be achieved by expanding the recruitment of converts to other, neighboring tribes. There should be nothing surprising in this; it is simply the other side of the process by which a group may reject a deviant by redefining group boundaries to exclude him. As such, it is a direct derivation from the theory of cognitive dissonance.

68 Festinger, Riecken, and Schacter, 1956.

In the cases of the Grass Dance cult, the Hand Game cult, the Prophet and Ghost Dance cults, and the Peyote cult, there was intertribal proselytizing among the American Indians.[69] Slotkin provides a large bibliography, and an eloquent statement of the conditions of anxiety, frustration, stress, in which these complexes of magical items could become important enough to engender the sequence of: (a) acceptance of the cult; (b) subsequent increase of dissonance and (c) dissonance-reduction by proselytizing.[70] In the cases of the five new magic-associations found among the Azande, we note that all of them were introduced from other tribes, and all came after the encroachment upon the natives' habitat by Europeans.[71] In the cases of the Baigona cult, the Manau cult, and the Taro cult with its several offshoots, there were attempts at intertribal proselytizing among the Papuans.[72] In the case of the Santo cult, there was proselytizing among the islanders.[73] In short, in every case of a new cult for which we have examined data, where any mention is given the means of the cult's transmission, there is found such proselytizing. Also, in every case, the cults seem to have followed the general sequence for which this paper has tried to account: (1) Innovation by a person or persons, usually by means of dreams, visions, or hallucinations. (2) Transmission to other persons, where the environmental context is one of stress, anxiety, frustration for the receptive group. (3) Accelerated transmission within the group, followed by inter-group proselytizing. In every case the contents of the cult-doctrine and practice seem to include items of coping and/or compensatory magic.

There is one final point of similarity in these native cults, which we would like to pursue merely as conjecture. Let us imagine a small tribe living on an island, in which a magic-cult arises. Let us say that the cult (or the given item of magic) is accepted by the entire group. But the magic is in regard to very important matters, and/or does not work too well; there still remains identical group dissonance. But the whole population has been converted; there are no other potential converts on the island. Is there any other means of adding social support to reduce the dissonance?

There is at least one way. One's ancestors were good tribesmen who would doubtless adopt the magic. If they could be brought back to life or if their spirits could be converted, this would provide additional social support. This would also

69 Siegel, 1955; Slotkin, 1956.

70 Slotkin, 1956, pp. 12-21.

71 Evans-Pritchard, 1937, p. 510.

72 Williams, 1928, pp. 3-37. Williams is not explicit about the Vailala cult. He simply states that, like the others, it spread very rapidly; it thus seems reasonable to infer some kind of convert-seeking.

73 *Idem*, pp. 100-101.

seem applicable to groups that face out-group domination, which produces stress, frustration, anxiety so that magic is developed and its use introduces dissonance, if the dominant out-group is superior in numbers to the subordinate in-group.

We note that a tenet of the Ghost Dance in all its many forms, was the impending resurrection of the native dead.[74] This was the case, too, for the Santo cult and the Manau cult.[75] McDonald reports a short-lived cult in which the notion developed that, if the cattle were killed, the tribal ancestors would rise from the grave and drive the English invaders into the sea.[76] The Vailala Madness cult is described as a "cult of the dead" whose priests were in constant communication with the ancestral spirits; the spirits were expected to return to the living tribesmen.[77] Finally, the Taro cult is described as, in its modern form, "not a worship of the taro [plant], but rather of the dead." And cult-members ascribe the *jipari* seizure to possession by a spirit of the ancestors.[78]

CONCLUSION

We may now formulate the main hypotheses of this paper. Taken together, these propositions form a fairly unified body of postulates which extend and give further articulation to Malinowski's analysis of the data on magic.

A. Where an individual or a society is confronted by environmental phenomena which produce on-going stress, frustration, or anxiety, there will be found attempts to cope with the phenomena in this sequence: (1) direct action intended to control the phenomena; (2) if the former is not successful, indirect attempts to control the phenomena—these may be magical means designed to achieve the original ends; (3) if neither 1 nor 2 is feasible, there may be attempts to compensate via supernatural practices.

B. When a group initially adopts such compensatory magic, an unstable situation is posited. We should expect to find that resort to 3 will be followed, or accompanied, by further resorts to direct action, or indirect action such as coping magic. If these attempts are still unsuccessful, we should expect the persistence of compensatory magic accompanied by abandoning of attempts to cope with the given phenomena, along with resignation, negativism, and the like.

C. If the environmental aspect which produces the stress, frustration, or anxiety is comprised of a dominant out-group and its sanctions, we may find negativism, resignation, etc., replaced by the adoption of the values and/or forms of the dominators. Ordinarily, this will not occur until direct and indirect coping have been attempted.

74 DuBois, 1939, pp. 131-140.
75 Williams, 1928, pp. 77, 100-101.
76 MacDonald, 1890, pp. 280 ff.
77 Williams, 1940, pp. 123-124.
78 Williams, 1928, pp. 10-11.

D. Where a culture faces such a stress-situation, the sequence of coping-compensatory behavior will tend to be initiated more often by those individuals least able to withstand the stress, so-called "unstable" individuals, than by "normal" persons. The same "unstable" individuals will reject the successive stages posited more quickly than the population-at-large. That is, they will tend to lead the movements which bring about the steps of the sequence.[79]

E. It is assumed that the adoption and use of a given item or items of magic by an individual or group involves the same sort of commitment as any other decision. That is, the belief in and use of an item of magic represents a cluster of cognitive elements equivalent to that involved in buying and smoking cigarettes.

F. To the extent that the magic fails to achieve its ends, there will be produced dissonance with the knowledges about continuing to use it. Where an item of magic is in general use, to the extent that the magic is ineffective, there will be dissonance generated in the owners of the magic. This is dissonance of the usual sort. Its magnitude is a positive function of the importance of the cognitions involved; its presence gives rise to pressures to reduce or eliminate the dissonance.

G. This dissonance may be reduced by the usual means: changing behavioral cognitive elements; changing environmental cognitive elements, physical or social; adding cognitions which enhance the value of the item of magic and/or minimize the importance of the dissonant cognitions.

H. Where new technology which invalidates the prior beliefs of a group is introduced, we should expect (a) initial resistance to the new item, and affirmation of belief in the magic appropriate to the phenomenon; (b) eventual adoption of the new technique and decline in the practice of that magic.[80] An innovation that does not invalidate prior beliefs should produce no such firm resistance.

I. Where a group possesses identical magic, the members of that group will tend to possess identical dissonance. In such a situation, social support will be relatively easy to obtain within the group. If the dissonance is not sufficiently reduced within the group, we may expect proselytizing to obtain converts; in this case the boundary of the group is expanded by redefinition to make erstwhile non-group members suitable for conversion.

It is our hope that scientists in the field will give tentative consideration to these hypotheses, so that additional empirical data with which to assess the merits and demerits of the postulates will become available.

79 See Barnett, 1941, pp. 160-171.

80 Because it is generally easier to deny a belief or a value, than it is to deny a physical fact, we should expect that innovations of a material sort, such as machinery, would be accepted more readily than non-material innovations, such as moral codes.

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A PREHISTORIC POPULATION ESTIMATE USING MIDDEN ANALYSIS AND TWO POPULATION MODELS[1]

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FOR THE GREATER PART of human history man gathered and hunted his food rather than producing it. As a food gatherer he stood in a similar relationship to his environment as do other mammalian groups. Clearly, the possession and elaboration of tools made him an eminently efficient hunter and gatherer, but his interdependence with his environment remained intimate. It seems reasonable that some of the ecological principles found applicable to non-humans might be fruitfully applied to man. Indeed, in the prehistoric foodgathering period artifactual remains are often so scarce that an ecological approach might be considered indispensable to understanding man's beginnings. Since foodgathering economies encompass a greater part of man's history, the problem of human ecology on this level falls almost entirely to the archaeologist.

It has long been recognized that population size, structure, and dynamics supply the primary data in ecological studies. At present no systematic methods exist for estimating the population size of prehistoric foodgatherers. Without quantitative data on population, ecological interpretations are severely handicapped. The purpose of this paper is to explore a method for determining the population size of a prehistoric foodgathering people as a first step in ecological analysis. In order to accomplish this, the physical analysis of the non-artifactual components of a midden in conjunction with two population models will be used.

The site chosen to explore the problem was LAn-174, Zuma Creek, California.[2] Wallace[3] has classified Zuma as Horizon II in the chronology of coastal southern California, characterized as it is by an abundance of milling stones, comparatively few projectile points, and a somewhat sedentary economy based on food collecting. If Zuma is typical of Horizon II, at least 4,500 to 5,000 years[4] must be assigned for its beginning.[5]

1 I would like to thank C. W. Meighan who made me aware of the potential value of midden analysis and M. Ascher for aid in the analysis of the quantitative data. I am grateful to the Department of Anthropology and Sociology, University of California (Los Angeles), for making available the facilities to do the work reported and to the Marblehead Land Co for access to Zuma Creek.

2 Peck, 1955.

3 Wallace, 1955, p. 219.

4 *Idem*, p. 221.

5 A recent C-14 run on a shell specimen from Zuma Creek supports Wallace's contention. I would like to thank Dr Carl L. Hubbs and the Scripps Institute of Oceanography, University of California, for running this test.

With regard to technique, column sampling was used. This technique, inspired by Kroeber,[6] pioneered by Gifford,[7] extensively investigated by Cook, Heizer, Treganza, and Greengo,[8] and most recently described by Meighan *et al.*,[9] has been applied principally in northern and central California. It has been shown that when certain assumptions are made about food intake and population size, the data gleaned from the physical analysis of column samples yield acceptable results as to the absolute age of middens. The aim here, as differentiated from the previous work, will be to compute population, not assume it. Experiments are reported which had as their goal the examination of certain aspects of the technique. Further, only an intensive occupation zone is submitted to analysis rather than the entire site as previously done, in order to achieve better control over the temporal dimension. Also, a method for reducing the number of samples needed is suggested. Of the two population models employed, the first is concerned with the correlation of surface area and population,[10] and the second views population as dependent upon fish-miles.[11]

Previous work has established that if the particular component in the midden to receive central attention is in sufficient quantity and finely subdivided, then between 15 and 30 samples (each weighing from 1 to 5 pounds) would suffice for accurate determination of midden components.[12] The central interest here was shell which appeared in large quantity and was finely subdivided. In the summer of 1957 twenty-eight samples were collected. Each consisted of a $4'' \times 4'' \times 6''$ rectangular column of midden taken from cores at varying depth, spaced along the recently exposed cliff wall (see Fig. 1.). After the samples had been dried, 2000 grams from each sample was washed in a $^1/_{16}''$ screen and all that the screen retained was washed and dried.

Research on the technique of column sampling never utilized a screen size of less than $\frac{1}{8}''$. According to Cook and Heizer the use of a screen size of less than $\frac{1}{8}''$ ". . . is disregarded as impossible to analyze without a prohibitive amount of labor and expense."[13] Nevertheless, it was felt that more work was needed on the implications of the use of varying screen sizes. Experiments were performed with four screen sizes: $\frac{3}{4}''$, $\frac{1}{4}''$, $\frac{1}{8}''$ and $^1/_{16}''$. The weights of the combined component samples, consisting largely of shell, bone, and rock, for all samples in

6 Gifford, 1916, p. 1.
7 *Ibid.*
8 Cook, 1946, 1950; Cook and Heizer, 1951; Cook and Treganza, 1947, 1950; Treganza and Cook, 1948; Greengo, 1951.
9 Meighan *et al.*, 1958.
10 Cook and Treganza, 1950.
11 Baumhoff, 1958.
12 Treganza and Cook, 1948, p. 292.
13 Cook and Heizer, 1951, p. 291.

each screen size were calculated. The results are shown in Table 1. Regularities were sought among the weights as the screen size was reduced. The relationships between the amounts retained in a ¼″ and ¾″ screen were hardly constant. The ratios varied from 6 to 33 having a mean ratio of 12.8. This may be explained by the fact that a single whole shell or a single rock might be retained in a ¾″ screen and its weight, although relatively high, was hardly representative. The fact that certain shells break up more easily than other shells and that shells generally break up more easily than certain rocks also might explain the large deviation from the mean ratio. The relationship between the amount retained in a ⅛″ and ¼″ screen size seemed more closely to approach a constant. The ratios

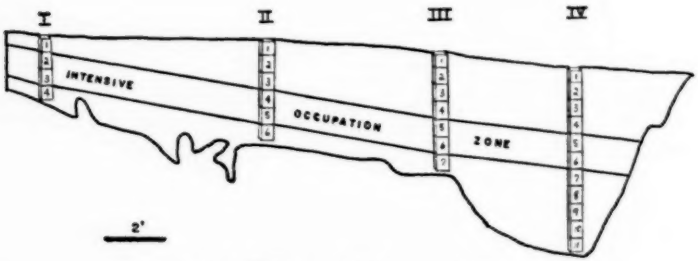

Fig. 1. Midden profile and excavated cores.

varied from 1.7 to 3.6 having a mean ratio of 2.7. In the third ratio calculated, the relationship between the 1/16″ and ⅛″ screens, the mean was 1.5 with a range of 1.2 to 1.7. In this final case, with a maximum deviation of 0.3 from the mean, a constant relationship could be said to exist. The error due to using a ¼″ screen instead of a ⅛″ screen is 64.3 percent and the error due to using a ⅛″ screen instead of a 1/16″ screen is 33.3 percent.

If a perfect screen size were used, the ratio between what it retains and a smaller screen retains would be 1. It may be noted that in the above experiment the relationships found are becoming more consistent, are decreasing, and are approaching 1. It might be expected from this progression that using a smaller screen size than 1/16″ we would continue to approach 1. If this progression continued, the mean ratio of the amount retained by any smaller screen to a 1/16″ screen would

TABLE 1

	¾"	¼"	⅛"	1/16"
SECTION I				
0-6	4	24	86	118
*6-12	9	70	186	274
*12-18	16	103	224	328
18-24	4	50	117	159
SECTION II				
0-6	3	28	76	120
6-12	5	31	90	143
12-18	1	20	70	120
*18-24	4	40	112	170
*24-30	5	44	110	164
30-36	1	19	60	97
SECTION III				
0-6	2	27	80	116
6-12	2	32	80	127
12-18	2	32	90	145
18-24	1	30	94	152
*24-30	6	51	136	204
*30-36	5	71	169	241
36-42	1	33	96	153
SECTION IV				
0-6	3	24	72	113
6-12	3	28	83	136
12-18	1	22	80	132
18-24	2	26	91	151
*24-30	3	54	168	255
*30-36	2	47	151	240
36-42	6	50	116	164
42-48	22	80	152	199
48-54	11	95	167	201
54-60	4	44	93	126
60-66	9	41	103	146

have to be less than 1.5. It may be concluded, at least in this case, that the use of a $^1/_{16}$″ screen was sufficient for reasonable accuracy.

This having been determined, eight samples were selected out of the total twenty-eight for analysis into component parts. The eight selected samples are starred in Table 1. The choice of the samples was based principally on artifactual data indicating that the site was intensely occupied during a certain era coeval with a one-foot zone of midden accumulation.[14] All of the samples in the selected series lie within this one-foot level. Further, it seemed unreasonable to attempt to determine the population, our ultimate objective, for the entire length of site occupation. Since there are no very accurate data on the rate of growth of a shell midden, by restricting our calculation to a single foot of intense occupation, the time approximation might be more reasonable. Finally, the consideration of a more limited length of time would aid in controlling the factor of rapid population change within the time dimension considered.

Consideration then shifted from the analysis of the comparative weight of the total combined component samples to the single component of shell. Having shown that a $^1/_{16}$″ screen was adequate for the total combined components, it remained to be demonstrated that the same could be said for the shell alone. In order to accomplish this, it had first to be established that the mean ratios of the total combined component samples and those of the selected combined component samples were sufficiently similar. The mean ratios of the selected samples are 11.9, 2.7, and 1.5. These mean ratios are identical with the previous ratios for $^1/_8$″ to $^1/_4$″ and $^1/_{16}$″ to $^1/_8$″. Using only the shell in the selected samples, the totals of which appear in Table 2, it was found that the mean ratios were sufficiently similar to those of all combined component samples to show that the change of screen size affects the shell component in much the same way as the combined components. The mean ratios for the shell were 10.8, 2.9, and 1.5. The fact that the greater differences are noted between the means of the total combined component samples, the selected combined component samples, and the selected shell component samples with the larger screens, supports the previous conclusion about increasing consistency of results with decreasing screen size.

The total amount of shell in the zone of intensive occupation may be approximated if the mean percent of weight of shell in the zone and the mass of the zone are known.[15] The mean amount of shell by weight as determined by the use of the $^1/_{16}$″ screen in the selected samples is 10.75. If the samples are truly represen-

14 Ascher, ms; Meighan, ms; Peck, 1955.

15 Cf. Cook and Treganza, 1947, p. 138.

tative of the zone, then it may be stated that 10.75 percent of the weight of the zone is shell. There are, therefore, 60,019,000 grams of shell in the zone.[16]

The overwhelming amount of shell in the zone is mussel.[17] If the figures 2.5 grams for each mussel, 1.065 grams of meat per mussel, and 7.5 grams of protein for each 100 grams of meat are accepted,[18] then it can be stated that 1,925,107 grams of protein were available from mussels to the inhabitants of Zuma Creek during the building of the one-foot zone under consideration.

TABLE 2

	¾″	¼″	⅛″	1/16″
SECTION I				
6-12	9	58	169	256
12-18	13	94	212	304
SECTION II				
18-24	4	30	96	150
24-30	5	34	99	148
SECTION III				
24-30	4	43	121	184
30-36	5	59	151	222
SECTION IV				
24-30	3	45	151	236
30-36	2	42	143	221

In order to convert the grams of protein into a form which would allow population estimates to be made, the amount of protein available will be equated to the grams of protein eaten by each person per day multiplied by the number of people times the number of days. We will assume 5 grams of protein per person per day. This assumption seems tenable on two bases: the large number of milling

16 The mass is a function of volume and density. Since the general outline of the zone exhibits dimensions similar to the segment of an ellipse, the formula $V = \frac{\pi a b l}{2}$ where a equals the length of the zone, b equals one-half the width of the zone, and l equals its height, is applicable. The volume of the zone is then 429.48 cubic meters. The density of the selected combined component samples, and presumably the entire zone, is approximately 1.3. The mass of the zone then becomes 558.32 metric tons. Multiplication of the percent of shell and the mass results in 60,019 kilograms or 60,019,000 grams of shell.

17 For a complete list of species that occur at Zuma, see Peck, 1955, p. 62.

18 Cook, 1946, p. 51.

stones and the paucity of faunal remains other than shell suggests that a much larger proportion of protein was obtained from vegetable matter; secondly, of the minimum 25 grams of protein per person per day requirement, only a small amount need be derived from a meat source in order to obtain adequate protein intake.[19] Substituting 5 for the grams of protein per person per day we have the equation:

$$1{,}925{,}107 = 5 \text{ (number of people) (number of days)}$$

This results in 1055 which equals the number of years times the number of people. It would now be possible to calculate either years of occupancy or population, given the other. However, it would seem more instructive to exhibit their relationship graphically in order to demonstrate the whole range of possible combinations. Figure 2 shows the range of both population or occupancy from 4 to 200. Within this range we can attempt a further approximation by estimating the limits of time needed to accumulate the one-foot midden and the limits of the population. The actual rate of growth of middens remains largely unknown. Data which might be of aid would be those derived from ethnographically analogous situations. Unfortunately, such data are lacking. We have, therefore, set an upper limit of 50 and a lower limit of 20 on the years it took for the segment of the midden in question to accumulate. Whatever scant support there is for this estimate is based upon the observations of C. W. Meighan among the Seri[20] and carbon 14 dates from lower and upper portions of two shell middens—one in Maine[21] and the other in northern California.[22] With regard to population, an upper limit of 100 and a lower limit of 4 has been set. This estimate of upper and lower bounds receives some support in those few studies which deal at length with foodgathering populations.[23] The limits on years have been indicated by vertical lines and the limits on population by horizontal lines in Figure 2. The portion of the graph on which these limits coincide can be seen by the crosshatched lines. The crosshatched lines show that in order to satisfy the equation arrived at from protein available and the imposed limits, the population can only vary between 53 and 21 where the years of accumulation can vary between 50 and 20.

Two population models are now introduced in order further to explore the results of the above analysis. The first model relates fishing-miles to population.[24]

19 *Idem*, p. 52.

20 Personal communication.

21 Bradley, 1957, p. 296.

22 Meighan, 1953, p. 5.

23 Carr-Saunders, 1922; Kryzwicki, 1934.

24 Baumhoff, 1958, p. 35. Fishing-miles or fish-miles, as used by Baumhoff, refers to statute miles along salmon streams.

The empirical data consist of the population and fishing-miles available to six California Athabascan groups. The cultural and geographical distance between the Athabascan groups and the people of Zuma, plus the fact that we are dealing with salmon fishing on the one hand and mussel gathering on the other, argue that

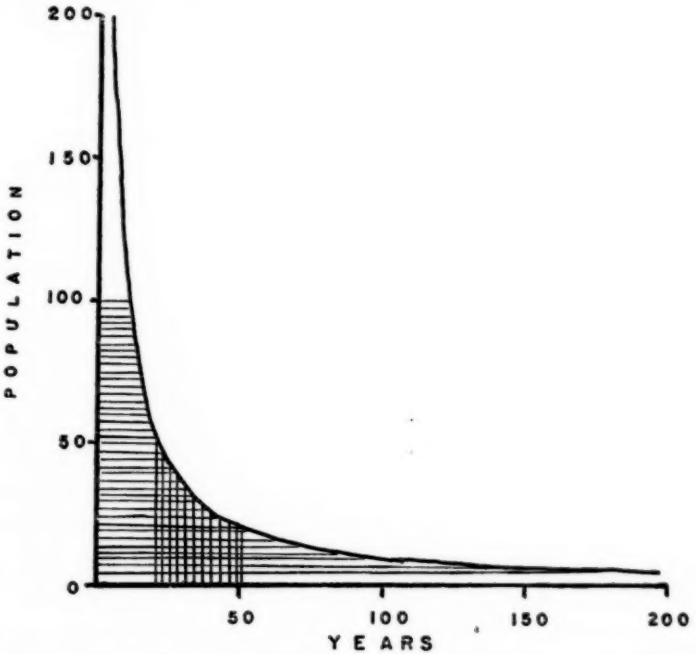

FIG. 2. Graph showing the relation of population and years with a fixed total content of 1055 grams of protein (population × years = 1055).

the model is non-applicable. Although the dangers involved in the application of this model are many, it is applied experimentally here because few other analogous models exist.

It should be noted that the equation used here is not that given in Baumhoff's equation 2. It is evident from Baumhoff's Figure 2 that log fish-miles should read ln fish-miles, and by the substitution of the data in Baumhoff's Table 1 into his

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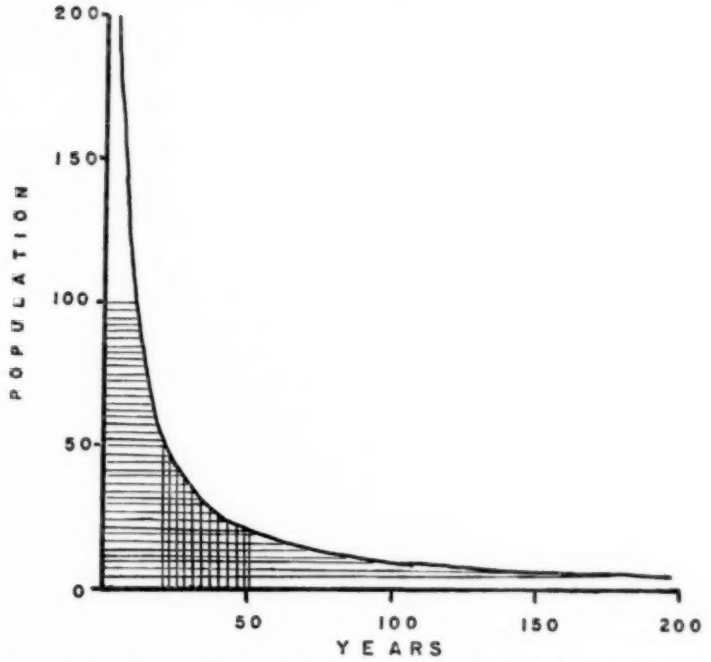

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equation 2, that an additional factor of 1000 is necessary in order to arrive at his results. The corrected formula reads:

$$\text{Pop.} = 1000\,[-1 + 0.75\ (\ln \text{fish-miles})\,]$$

Presently, mussels occur along a four mile frontage at Zuma.[25] Assuming that the inhabitants at Zuma Creek had the advantage of the same frontage for mussel gathering, it may be concluded, by substitution in the above equation and disregarding the error term, that the population at Zuma during the time of intensive occupation was 40.

The Cook-Treganza model[26] views surface area as a correlate of population. It shows that a relationship exists of the form:

$$\log \text{pop.} = \text{constant 1} \times \log \text{area} + \text{constant 2}$$

The constants are arrived at by fitting the best straight line to data from four central California archaeological sites. All four sites are foodgatherer sites on a similar cultural level and hence the constants are directly applicable. Constant 1 is given as 0.361 and although constant 2 has been omitted, it has been reconstructed to be 0.342. The result of the application of this model to Zuma is 30 people.

As a result of midden analysis, the population of Zuma during a period of intensive occupation was fixed as lying somewhere between 53 and 21 people. The advantage of stating conclusions in a range instead of in terms of an absolute value is that it takes into account the inaccuracy of the original data and permits selection from within the range on other bases. Further, a site occupied seasonally may be expected to vary in population from year to year. The population figures calculated as a result of the two models were 30 and 40. Although the models may not be directly applicable, the fact that they are close to one another, and that they fall within the range found as a result of midden analysis, is worthy of note.

CONCLUSIONS

Experiments with the technique of midden analysis demonstrated that increasing consistency is achieved with decreasing screen size. Although this generality might have been predicted before we began, the magnitude of the change, and the change of consistency due to the variation in screen size, was not. We submit that at our present state of knowledge, the best screen size should be determined in each case, since an arbitrary choice may lead to gross inaccuracy. Clearly, more

25 Ascher, ms.

26 Cook and Treganza, 1950, pp. 231-233.

experiments are indicated. We are in sympathy with the important considerations of time and funds,[27] but our sympathy in this respect is overweighed by our concern with the necessity that scientific archaeology follow the rigors of scientific method.

We were able to reduce our original sample of 28 to 8 samples after demonstrating that the 8 samples selected were sufficiently similar to the 28 samples. It was also demonstrated that shell was affected by screen size in much the same manner as the combined component samples. These data, applicable only to Zuma, do not permit wider generalizations. We suggest, however, that the reduction in the size of the original sampling may be accomplished if similar archaeological circumstances and arithmetical regularities are found.

By considering only the intensely occupied zone at Zuma, we were able to achieve a better control of time. Such control is crucial if we are to achieve dependable population estimates. If it is true that populations change through time, then it is essential that time approximations fall within reasonable limits. Further, if a midden component may be used in conjunction with artifactual evidence to distinguish varying intensities of occupation throughout a midden deposit, then by the separate analysis of each such designated zone, population change over a span of time may be determinable.

If we are ever to make significant statements about human ecology, population figures will be necessary. To make dependable population estimates, assumptions should be replaced with empirical data. It therefore behooves archaeologists working with ethnologists, and ethnologists themselves, to make those observations and collect those data which are crucial for archaeological analogy.

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SOME NOTES ON ACOMA KINSHIP TERMINOLOGY

WICK R. MILLER

THE KINSHIP MATERIAL presented in this paper[1] was obtained between 1955 and the present from an Acoma woman about thirty-five years old who now resides in San Francisco, California. In spite of the obvious limitations on such a field situation, the material thus far collected reveals a few clear errors as well as omissions in previously published kinship lists.[2]

Our list agrees on almost every point with Parsons except for a few omissions in her lists. Kroeber points out the probable shortcomings of his list, the most important of which is the mixing of Acoma and Laguna terms. Mickey has shown that, in some cases, previous recorders have failed to distinguish sex of the speaker (properly, sex of the person referred to by the pronominal prefix) and also failed to distinguish reference and address terms. There are several points of difference between Mickey's list and our list for terms used by male ego. We have been unable to check our material directly with a man, but (1) several of the terms occur in texts, tape-recorded by men and (2) some terms we could check by getting the reciprocal. (It should be pointed out that Mickey worked with only two informants, a man and a woman.)

The phonemic transcription used below has been described in a previous article.[3] Inalienably possessed kinship terms are given with the first person pronominal prefix in parentheses *(s'-)*. In all but the one case noted below, we have changed earlier Acoma transcriptions to conform with ours.

Reference Terms. The grandparent-grandchild group consists of three self-reciprocal terms which distinguish the sex of both speaker and person spoken of. *nána*, used by men (borrowed from Zuñi) : MoFa, FaFa, DaSo, SoSo. *dʸa²áu*, used by women: MoMo, FaMo, DaDa, SoDa. *bá·ba'·*, a cross-sex term: a man's MoMo, FaMo, DaDa, SoDa; a woman's MoFa, FaFa, DaSo, SoSo. Mickey and Parsons found the same terms and usages.

The parent-offspring group distinguishes sex of the person referred to, but (in most cases) not of the speaker. *n'a'isdʸíy'a:* Fa, FaBr. *n'â·ya:* Mo, MoSi, FaSi; a

1 Grateful thanks are due David M. Schneider and Mary R. Haas who read the manuscript and made several helpful suggestions. The fieldwork was conducted under the direction of the Department of Linguistics, funds provided by the Department of Linguistics and the University Patent Fund, University of California, Berkeley.

2 Kroeber, 1917, pp. 83-87; Parsons, 1923, pp. 199-201; 1932, p. 380; White, 1930, p. 40; Mickey, 1956, pp. 250-251.

3 Miller, 1959. The symbol ǯ is substituted for a tailed ʒ with inverted caret, ʒ for tailed ʒ.

man's SiDa. *(s')ámə'·tʸi:* So, BrSo; a woman's MoBr, SiSo. *(s')ámaʔákə:* Da, BrDa; a woman's SiDa. The self-reciprocal term *ʔá·náwé* is used for a man's MoBr, SiSo. Our terms and their usage agree with the lists secured by Parsons except that she does not give the terms for SiDa, nor for a woman's MoBr, SiSo. There are several points of difference between our list and Mickey's list. She lists (1) a man's SiDa as *s'ámaʔákə* ("my daughter"); (2) a man's SiSo as *s'ámə'·tʸi* ("my son"); (3) FaBr as *ʔá·náwé;* and (4) *ʔá·náwé* as MoBr for both men and women, although in her diagram Mickey gives *s'ámə'·tʸi* for a woman's MoBr. Mickey notes some of the discrepancies and explains them as a shift to bilaterality. Our material was carefully checked, and we strongly feel that Mickey is in error on these points.

The sibling group consists of four terms that distinguish the sex of both persons: *dʸúm'ə* "brother of a man"; *(s')ak'ûiça* "(my) sister of a man"; *(s)aʔáu* "(my) sister of a woman"; *(s')áwaçə* "(my) brother of a woman." The sibling terms are also used for parallel cousins. These terms and their usage agree with those listed by Parsons. Kroeber also lists *(a)kwi* "sister of a man," *(a)wa* "brother of a woman," and *am* "sister of a woman." Mickey points out that the first two forms are specifically Laguna terms—they are probably morphologically related to the Acoma forms—but the last term has not been recorded by anyone else for either Acoma or Laguna. Mickey gives *s'áwaça* for both a man and woman's brother and gives *dʸúm'ə* as an address term only. This is most certainly an error. We have checked the form many times and *dʸúm'ə* has been recorded in texts several times in contexts that demand reference terms rather than address terms.

There are two terms for clan relatives: *(s')â·wi* "(my) clansmen," and *(s')ak'ú·yá* "(my) father's clansmen." The terms seem to apply to any clan member, regardless of sex or residence. The second term has been recorded at Laguna for FaSi by both Kroeber and Parsons.

(s')á·çə and *(s')âuk'ui* are used for "(my) husband" and "(my) wife," respectively. Expressions such as *ʔíy'aça gá·nâ·yaši* "the children's mother" are also used. This practice is also found at Laguna.[4]

Eggan states: "Men who marry into Ego's lineage in his generation and below are classed together as *wati* [*wa'·tʸi*] . . .; women who marry men of the lineage are *piye* [*bíhí*]."[5] This statement appears to be essentially correct, though there may be alternate terms in some cases. Thus MoBrWi is either *s'ámaʔákə* "my daughter" or *s'abíhí* (the reciprocal is either *sin'â·ya* "my mother" or *sk'ubíhí*, a verb meaning "he, she, is *'bíhí'* to me"). The same type of variation is found for MoSiHu and FaBrWi, but for FaSiHu only *sin'a'isdʸíy'a* "my father" (the re-

4 Parsons, 1923, p. 163.

5 Eggan, 1950, p. 228.

ciprocal *s'ámaʔáke* "my daughter") is used, never *s'awa'·tʸi* or the reciprocal *sk'uwa'·tʸi*.

There is no term for a child's parent-in-law. Expressions such as *s'ámə'·tʸi c'iwa'·tʸi* "my son's in-laws," *s'amaʔáke c'ibíhí* "my daughter's in-laws" are used instead. Apparently the sex of the speaker or the person spoken of is not important.

There is some confusion about the terms for cross-cousins. Kroeber found that paternal cross-cousins were called "mother" and "father," maternal cross-cousins "daughter" and "son." White also gives "daughter" and "son" for maternal cross-cousins, but sibling terms for paternal cross-cousins. Parsons lists sibling terms for all cross-cousins. Mickey found that her male informant used sibling terms for all cross-cousins, her female informant used sibling terms for paternal cross-cousins but grandparent terms for maternal cross-cousins. Eggan[6] reconstructed grandparent terms for a woman's paternal cross-cousins: *bá·ba'·* for a woman's MoBrSo and *dʸaʔáu* for a woman's MoBrDa. But he did not reconstruct grandparent terms for the reciprocal, e.g. *bá·ba'·* for a man's FaSiDa. In our material whenever a grandparent term is used the same term is used for the reciprocal; whenever a parent term is used an offspring term is used for the reciprocal. Parsons reported the same situation at Laguna.[7] Mickey explains the discrepancies in her material by postulating a shift to bilaterality. This may be true in part, but there is not yet enough material available to draw firm conclusions.

Unfortunately our material sheds little light on this problem. Our informant and her brother refer to their FaSiSo as *n'a'isdʸíy'a*, "father," and the reciprocal is *s'ámaʔáke* "my daughter" and *s'ámə'·tʸi* "my son." It may be significant that this individual is considerably older than our informant and her brother. The informant and her brother use sibling terms for their FaSiDa, and sibling terms are used for the reciprocal. The informant's MoBr has no children and she was reluctant to say what the terms might be since she might "make a mistake." However she stated that her children and her brother's children would use sibling terms regardless of sex. (It is noteworthy that the informant was reluctant to give terms for relatives that did not exist if they were in her generation or above, but readily gave the terms for lower generations.) She also said, when questioned, that grandparent terms could be used for her cross-cousins, except for those relationships that called for "father," "daughter," and "son." Grandparent terms are also used between the informant's children and her FaSiSo (e.g. a man or woman's MoFaSiSo and a man's MoBrDaDa, MoBrDaSo).

It is clear that there are now, and probably have been for some time, alternate terms for cross-cousins. More field work is needed to establish the limits and the

6 *Idem*, p. 226.

7 Parsons, 1923, p. 173.

nature of the alternates. It may be that the alternates are similar to those that Schneider and Roberts found at Zuñi.[8]

Address Terms. Address terms differ from reference terms in only a few places. *ží·ži'·* is used in address where *s'ak'ûiça* and *s'áwaçɔ* are used in reference; in other words, *ží·ži'·* means "sibling (cousin) of the opposite sex." *ʂa'áu* "my sister" (of a woman) has a special vocative used in address, *ga'áu,* which is homophonous with the third person possessed form "her sister." (The word is also irregular in that it uses a first person plural prefix [*ʂ-*] for the singular.) These terms and their usage correspond exactly with Parsons' terms.[9]

'áwá'áwá is used in address between a man's SiDa and a woman's MoBr (self-reciprocal). Although the term has not been reported in the literature before, I am reasonably certain that it is not just a "family word." White lists *si nayatcani* (= *sin'â·yaʐán'i,* "my foster mother") and *saawa* for SiDa. The second term may be our *'áwá'áwá;* however the presence of *s-,* first person pronominal prefix, is puzzling.

Kinship terms are used to address affinal relatives. Thus "sister-in-law" is *ga'áu* (woman speaking), *ží·ži'·* (man speaking), and so on. Parents-in-law of one's child are addressed by sibling terms. Women may also be addressed by *bî·y'ai,* vocative of *bíhí;* the term can be used in addressing both wives of relatives and female relatives of one's husband. It may be that *bî·y'ai* is more commonly used for woman in lower generations.

n'â·ya "mother" has a special vocative form *n'ayé.* It is said to be used "only with your real mother." However, *n'ayé* is also used by the informant for her MoSi because she feels "very close" to her. Compare also *nayé·ʐá* "married woman" (who has children), and Parsons' remarks about Laguna.[10]

Personal names are employed in address of husband and wife. "For fun," a husband may be addressed as *hástⱽi* "old man," a derogatory term in other contexts. When the husband is older and "the children are grown up" he may be addressed as *hasdⱽî·ze'··ši,* a non-derogatory term for "old man."

All other address forms are based on the reference forms. Alienably possessed terms are used with the first person pronominal prefix and inalienably possessed terms are used with or without the prefix. However, *s'ámɔ'·tⱽi* "my son," an alienably possessed noun, must be used with the prefix; *mɔ'·tⱽi* means "boy, young man" and is not a kinship term. Compare also *s'áma'ákɔ* "my daughter," irregularly derived from *magɔ'* "small girl."

General Remarks. The terms *ží·ži'·* and *'áwá'áwá* are not found found in any

8 Schneider and Roberts, 1956, pp. 15-18.

9 Parsons, 1923, p. 200.

10 *Idem,* p. 148.

other Keres pueblo as kinship terms. Parsons notes that the term "*chichi* . . . is at Laguna merely a nursery term, used to or by children."[11] In a previous article we have shown that the term is irregular in the Acoma linguistic system since it contains the phoneme /ž/, otherwise found only in Spanish loanwords.[12] Hence the term was probably borrowed from "baby talk" after the Spanish contact. Very likely *ʔáwáʔáwá* is from baby talk also. Both terms have a phonetic shape that is common in baby talk and rare in adult words. Compare the following baby words: *dʸá·dʸa'·* "daddy," *yá·ya'·* "mamma," *má·ma'·* "carry on back," *nyá·nya'·* "nurse," *ʔák'aʔák'a* "drink," *ʔám'aʔám'a* "go to toilet."

It seems reasonable to postulate that the address terms for siblings changed on the analogy of the grandparent terms. Two of the sibling terms, *gaʔáu* "sister" (of a woman) and *dʸúm'ɔ* "brother" (of a man), are by nature self-reciprocal. Thus *ží·ži'·* functions as a cross-sex sibling term in the same fashion as *bá·ba'·*, cross-sex grandparent term. *ʔáwáʔáwá* is also a cross-sex term, and in addition emphasizes a woman's position in the clan in the same manner as *ʔá·náwé*.[13]

These are not the only examples of kinship terms being borrowed from baby talk. At Cochiti and San Felipe the term for "mother" is *yaya*[14] cognate with the Acoma baby word *yá·ya'·*. The term *bá·ba'·* is also suspect because of its phonetic shape, even though it is pan-Keres.

There are only two major differences between the Acoma and Laguna kinship systems. One is the difference in cross-cousin terms; the Laguna consistently use grandparent terms for this class of relatives. The other is the use of *s'ak'ú·yá* for "my father's clansmen" at Acoma, but "my father's sister" at Laguna. The Laguna usage is probably older since the term is formed from the root *k'ú* "female, woman." The terms *s'âuk'ui* "my wife" and *s'ak'ûiça* "my sister" also contain the same root.

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11 *Idem*, p. 168.

12 Miller, 1959.

13 Acoma, *ží·ži'* and *ʔáwáʔáwá* correspond to the Rio Grande Keres terms *meme* and *nyenye*, except that the latter terms are used in both address and reference (Parsons, 1923, pp. 202-203).

14 *Idem*, p. 168.

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ITZAMNA COCAHMUT, THE POSSIBLE "SPARK-BIRD" GOD OF THE MAYA[1]

FLOYD THURBER AND VALERIE THURBER

THAT THE MAYA SUN-GOD Itzamna took the form of a bird for one of his manifestations, and that this mythological bird, in Maya concept, may have carried the sun-god's spark of human life from the heavens to earth, seem indicated. Development of this hypothesis has produced factors to support such a concept, and the findings appear worthy of consideration. (Comments appearing within brackets have been supplied.)

Thompson states, "Itzamna's alternative name of Yaxcocahmut might even have reference to the celestial birds so intimately associated with the sky monster in art, since mut means 'bird' in some Maya languages. . . ."[2] Spinden comments on this point, noting, "The possible connection between the Roman-nosed God and a bird of some sort has already been mentioned."[3] As noted, Itzamna was depicted as a sky monster, and Tozzer and Allen speak of ". . . the 'bird serpent'."[4] Thus a relation between Itzamna and some form of bird appears to exist.

Roys reports, Yax-Cocay-mut (Tizimin) "might be translated as 'the green fire-fly bird'."[5] Motul cocay (cocah) can mean "glow-worm that shines at night," but since mut can mean bird, cocah-mut suggests a "bird that shines at night." As noted, Roys calls it a "fire-fly bird." The foregoing appears to identify Cocah-mut as a mythological bird that shines at night with a small spark like a glow-worm or a firefly.

This conclusion finds supporting factors, and takes the discussion to Thompson's fire-drill glyph shown in Figure 1, no. 1, and to its postfix shown in no. 2. Of this glyph, Thompson states, "The postfix, as noted, may be a stylized picture of smoke or, perhaps fire."[6]

This leads to consideration of the final element of the postfix, as shown in no. 3. It can be seen that this final element may represent the Maya concept for the delineation of a small spark, such as might be generated with a fire-drill, the

1 Acknowledgment is gratefully made to Dr J. Eric Thompson for his consideration of material used in this report and for his helpful comments.

Arrangement of material was greatly facilitated through the assistance given by Dr Spencer L. Rogers, Scientific Director of the San Diego Museum of Man.

2 Thompson, 1939, p. 153.

3 Spinden, 1957, p. 80.

4 Tozzer and Allen, 1910, p. 314.

5 Roys, 1933, p. 153, n. 5.

6 Thompson, 1950, p. 265.

central circlet possibly indicating the spark itself, and the surrounding ring of smaller dots probably depicting the circle of light radiating from the spark.

Tentatively accepting the spark for working purposes, some evidence is available to show that the Maya scribes may have used this spark symbol to identify

FIG. 1. Maya hieroglyphs considered in the discussion of Itzamna Cocahmut.

both a spark of fire and the spark of human life from heaven. Support for this concept requires factors to indicate a relationship between Itzamna and the long-nosed rain god Chac (Schellhas God B).

Thompson supplies information on this point, reporting, "If our identification of Itzamna as the celestial monster be correct, then . . . Itzimt'ul Chac would be another aspect of the sky monster," and further, "The Chacs, like the Itzamna's,

are rain gods. It is possible that they merely represent a different manifestation of the Itzamnas. . . ."[7] Thus Itzamna and the long-nosed rain god Chac may equate.

Similarly, the discussion needs to show some relationship between Chac, the rain god, and Maya hieroglyph Ik, the second-day sign of the Maya "month." Thompson records, "The day-sign Ik, therefore, represents Schellhas God B, god of rain in particular, and of life, germination and fruitfulness. . . ." Thus, Itzamna, Chac, and the hieroglyph Ik appear to equate properly.

Concerning the meaning of the symbol Ik, Thompson states, "Ik . . . means not only wind but breath and by extension life itself. . . . The name glyph then might have a meaning, such as 'giver of life'. . . ."[8] The priest of Chumayel gives Ik as ". . . the breath of life. . . ."[9] Spinden notes, ". . . Itzamna was regarded by the Mayas as the creator and father of all. . . ."[10] Schellhas reports that Itzamna ". . . seems . . . to be connected with birth."[11] The Popol Vuh, the Sacred Book of the Quiches, speaks of the people ". . . raising their faces to the sky when they ask for daughters and sons. . . ."[12] In the Guatemalan highlands, Bunzel records, "Parents always accept children . . . as the gift of God. . . ."[13]

Thus Itzamna appears to be "the giver of life," supplying the breath of life that may produce the spark of life for humans. This concept finds strong support.

For example, in C. Dresden 7b (see no. 4), the picture shows the Roman-nosed god Itzamna, holding up to his mouth what is probably a bird's bill. The significance of "the bill of a bird" is evidenced by one occult ritual in the Guatemalan highlands which calls for "the bill of one Quetzal for the Cuchqajau [diviner]."[14] In the picture, the spark element of the fire-drill glyph can be seen just in front of Itzamna's lips. His open mouth suggests that he could be applying "the breath of life" to the bird's bill.

Confirmative support for this idea appears to be presented in the following picture in C. Dresden 7b (see no. 5), where a bird is shown descending to earth with the spark element as an infix in the middle portion of its bill. The glyph from C. Madrid 10b, shown in no. 6, is a text glyph that may depict Itzamna-cocahmut, the "spark-bird," with the "spark of life" infixed in the mid-portion of its bill. Thus, it appears possible that the "spark-bird" may be the symbol for Itzamna-cocahmut.

7 Thompson, 1939, p. 153; 1954, p. 228.
8 Thompson, 1950, p. 73.
9 Roys, 1933, p. 117.
10 Spinden, 1957, p. 302.
11 Schellhas, 1910, p. 23.
12 Goetz and Morley, 1950, p. 172.
13 Bunzel, 1952, p. 99.
14 *Idem*, p. 338.

It is of interest to note that Landa shows a glyph that may be the spark-bird (see no. 7) with an Akbal infix in its forehead, as the symbol for the Maya month Xul.[15] In this case, the spark element is infixed at the back corner of the bird's mouth.

Any findings that may help in clarifying the gods of the complex Maya pantheon are sorely needed, and the foregoing considerations are presented as tentative factors for further study.

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15 Tozzer, 1941, p. 157.

THE APPEARANCE OF THE MOUNTED INDIAN IN NORTHERN MEXICO AND THE SOUTHWEST, TO 1680

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CERTAINLY one of the most significant phenomena of post-Columbian American Indian history is the spread of horses and mules and the appearance of mounted Indians. In spite of its importance to archaeologists, anthropologists, and historians, however, this subject has heretofore been dealt with only in a second-hand and unsatisfactory manner insofar as the area of the Southwest and northern Mexico is concerned. Certain scholars[1] have published monographs on the spread of the horse but on the whole for this area they have been based upon the examination of only a limited amount of historical documentation of a secondary nature or else they have dealt with only the final phases of the spread of the horse. In this paper it is proposed to examine Spanish documents from various archives in the United States, Spain, and Mexico and from these sources to trace as well as possible the appearance of the mounted Indian in northern Mexico and the Southwest.

Many scholars of the early 1900's such as Clark Wissler assumed that the horse was acquired by the Plains Indians as a result of strays from the Coronado expedition of 1540-1542.[2] Later writers generally discounted this theory and found their answer to the problem in the Spanish colony of New Mexico founded in 1598. For example, Francis Haines favored the thesis that conquered Pueblo Indians were put to work herding livestock and subsequently escaped to unconquered groups carrying their knowledge of horse-care and riding with them. By the 1650's the Navaho were taking horses by force and by the 1660's the Plains Apaches were acquiring them by trade. The spread of the horse was then given great impetus by the Pueblo Revolt of 1680 and horses were acquired by tribes as far away as eastern Texas by 1690.[3]

Certainly, as Haines points out, the appearance of the mounted Indian cannot be accounted for by a few isolated strays (and we do not know that there were any strays) from the Coronado expedition. Likewise, Haines' thesis that horse-riding must have been spread by Indians who had learned to ride from the Spanish must in general be accepted. The present author, however, does not feel that New Mexico was the earliest source of horses for the Southwest nor the only source in later

1 Roe, 1955; Haines, 1938; Wissler, 1914; Alexander, 1939; Ewers, 1955; and others.
2 Wissler, 1914, p. 9.
3 Haines, 1938, p. 431.

times. The horse's spread northward was a gradual one and the Spanish sources indicate that by the 1590's horses were reaching what it now the United States at many points and as a result of phenomena common to most of northern Mexico.

NORTHERN MEXICO: 1551-1608

In 1542 the Spaniards were forced to allow their Indian allies to use horses in order to put down the Mixton Revolt in the Jalisco area. The rebels were able to acquire horses also from the Spanish *estancias* in the Guadalajara region and although the revolt was suppressed many individual Indians were able to keep up their raiding and to retreat northward.[4] In the same year of 1542 mineral wealth was discovered at Zacatecas and thousands of Spaniards, Mestizos, and semi-Hispanicized Mexicans and Tarascans poured into the north taking horses and mules with them. The Zacateco Indians and their allies, the Guachichiles, Guamares, and Pames (called collectively the Chichimecos) were soon goaded into war, as the Mining Frontier of New Spain was a vicious phenomenon highlighted by the practice of Indian slavery. The Chichimecos at first waged only a defensive war but by the 1550's they had gained enough experience to carry on successful raiding of a serious nature. In 1551 the Guamares raided an estancia and carried off livestock. In 1554 the Indians attacked a wagon train of sixty wagons, carried off 30,000 pesos worth of booty and used mules to carry the goods away. By 1580 the Chichimecos were mounted Indians with plenty of horses and other livestock, and the Chichimeco War raged on.

It was not necessary for the Indians of north central Mexico to steal from the Spanish in order to acquire horses, however, for in 1579 Juan Suárez de Peralta reported that in New Spain ". . . there are today a very large number of horses and mares, so many that they go wild in the country, without owner, which ones are called cimarrones." In fact, there were wild horses who lived for twenty years without having an owner.[5] By 1580 the Indians of the area of north central Mexico had such easy access to mounts that the Spaniards were willing to give them horses in order to bribe them into accepting peace. It is to be understood that the Spanish would never have given horses to their enemies unless the latter had the possibility of getting them from another source. A report of 1582 declared that the Chichimecos had more horses than the Spanish and that the Guachichiles were trading livestock to the interior. In one valley the Indians had corrals and rode herd on thousands of cattle which they had stolen. A report of 1585 declared that

4 Powell, 1952, pp. 159, 290.
5 Suárez de Peralta, 1579, p. 324.

". . . they are no longer content to attack the highways on foot, but they have taken to stealing horses and fast mares and learning to ride horseback. . . ."[6]

Thus it is clear that in the years from the 1550's to the 1580's the Indians of the region lying between Zacatecas and Saltillo acquired horses, mules, and cattle and learned to use the former for riding. There are several possibilities how the Chichimecos learned to ride. They may have learned by watching the Spanish and then mastering the art by trial and error, or they may have learned from anti-Spanish Mexicans or Tarascans who had joined their ranks, or they could have mastered the art by contact with the many bands of runaway Negroes and mulattos who were raiding in the region. In all probability all of these factors operated to produce the mounted Indian of north central Mexico.

The Mining Frontier was an expansive, explosive thing. It soon leaped beyond the old Chichimeco region carrying exploitation and warfare to regions in present-day Durango, Chihuahua, Coahuila, and Nuevo León. Virtually all of northern Mexico became a *tierra de guerra*, a land of war, long before 1600. Let us first of all examine the area of Coahuila and Nuevo León.

As early as the 1550's the mines of Mazapil were opened up near Saltillo and soon afterward the latter settlement was founded. The Indians of that area (Guachichiles) were involved in the Chichimeco War and may have acquired horses quite early but we have no definite information until about 1580. At that time Captain Gabriel Ortíz Fuenmayor was commissioned to pacify the Guachichiles between Zacatecas and Saltillo by ". . . giving freely of his goods (great quantities of food, clothing, horses, colts, and many other things). . . ."[7] In 1582 one of the earliest missions in the north was established at Saltillo, but the Chichimecos revolted and it was not re-founded until 1592. During the years 1582-1592 the rebels committed many robberies between Saltillo and the surrounding regions.[8] Actually the Guachichiles and other Indians of the Saltillo region rebelled many times in the 1580's and on one occasion (c. 1587) they robbed many horses and mules from the Spaniards.[9] Thus it is clear that by the mid-1580's the Indians of southern Coahuila were in possession of horses and presumably riding them since they had had plenty of opportunity for learning the art.

In 1579 Luis de Carabajal was commissioned to settle the Nuevo León region and León (Cerralvo) and Almaden (Monclova) were soon founded. From the outset slave-raiding was the favorite occupation of these Spaniards and most of the neighboring tribesmen soon became hostile.[10] Finally in the 1590's León was

6 Powell, 1952, pp. 50, 175, 205, 210.
7 *Idem*, p. 205.
8 Arlegui, 1737, p. 17.
9 Urdiñola, 1587 and 1591.
10 Bolton, 1912, p. 14.

abandoned due to the constant Indian raids and the Spanish retired to Saltillo. In their wars the Indians had carried off goats and horses, for our present purpose a very significant thing since León was a scant fifty miles from Texas.[11]

Almaden was likewise an ill-fated colony and in 1590 its commander, Gaspar Castaño de Sosa, decided to move the settlement to New Mexico. On the Río de Nadadores, to the north of Almaden, a number of horses were carried off by Indians in July, 1590.[12] By 1590 then, the Indians of the Monclova-Cerralvo region were capable of acquiring horses and in all probability of riding them. Horses continued to be acquired by the Coahuila natives during the balance of the sixteenth century. In 1591 the Saltillo area Indians resumed raiding and in 1595 the Guachichiles and Coahuilas were brought to peace.[13] From 1603-1606 a mission was established at Monclova but it was destroyed by Indians (said to be Tobosos) in the latter year and we may suppose that the livestock of the mission was in part acquired by the Indians.[14] By 1608 Indians as far north as the border of Texas were involved in hostilities with the Spanish. The Quamoquanes killed a priest and three Indian *doctrineros* and then called for a general revolt of all the tribes north of Saltillo and Parras. The Spaniards were forced to campaign to the junction of the Río Sabinas and Río Bravo (Rio Grande) in order to bring the rebels to peace.[15] In the same year the Indians of the Mazapil region revolted and forced the miners and settlers to flee the area.

It appears then that the Indians of Coahuila and Nuevo León began to acquire horses and mules by 1580 and by 1590 horse-acquiring had nearly reached the border of Texas. The natives then continued to get mounts in the late 1590's and early 1600's.

The spread of the horse into the areas of Chihuahua and Durango or, if you wish, Nueva Vizcaya, was a result of the expansion of the Mining Frontier to that region. In 1554 Francisco de Ibarra left Zacatecas to open up new mines in the north and by 1560 the settlements of Santa Barbara, Indehé (Indé), and San Juan del Río were producing revenue and spreading hatred, bloodshed, and slavery to the area of southern Chihuahua. The Indians of the new area were goaded into hostility and Ibarra was continually forced to quell rebellions. In 1561 the Tepehuanes of Durango were in league with the Chichimecos, and the natives of San Juan del Río revolted at about the same time. The latter killed many Spaniards and Indian laborers and did so much damage that Ibarra himself had to deal with them. Subsequently, the San Juan natives rebelled again and killed some 400

11 De León, n.d., pp. 95-98.
12 Castaño de Sosa, 1590, p. 284.
13 Urdiñola, 1591 and 1595.
14 Descripcion de la Villa de Monclova.
15 Urdiñola, 1608.

horses.[16] Warfare was almost continual in Nueva Vizcaya and we know that by the 1570's the Indians were acquiring horses. In 1574 a Spanish frontier soldier serving in Nueva Vizcaya reported after making a number of campaigns

> by information [gotten] from some of them [enemy Indians] whom at times I have captured, I have received reliable [news] that, a hundred leagues inland to the north, there is a great population of natives who treat and trade with the said Chichimecas and encourage them and give them aid and assistance in order to commit the said injuries [to the Spanish]. They barter profitably with hides and metals rich in silver (this is what the said Chichimecas use to decorate and paint their bodies) for the mules, horses, Christian Indians and slaves which they [the Chichimecas] steal in this way. It will be of much benefit to your royal service for that people to be discovered, conquered and brought under your royal service. They have the name Quibira.[17]

A hundred leagues to the north of the Nueva Vizcaya frontier would be in the vicinity of La Junta (the junction of the Rio Grande and Conchos) and the Julimes and Cholomes Indians of the latter area were probably the "great population" receiving slaves, horses, and mules from the rebel tribes of Nueva Vizcaya. It is to be understood that the term "Quivira" was used merely in vague reference to a rich northern region (coming from the Arab word Kbir or Kebir and the subsequent Spanish form Quivir meaning "great" or "large"). Thus as early as 1574 horses and mules were being traded to the southern border of west Texas.

The Indians of Nueva Vizcaya became increasingly able warriors as the years passed by and in 1575 it was reported that the rich mines of Indehé had been entirely depopulated and that a *desploblado* extended along the Río Nazas to the Río Florido. There were *haciendas de labor* in operation on the Río Florido and at Santa Barbara but continual warfare was making the mines of the latter place impossible to work.[18] In the 1580's the Tepehuanes and Zacatecos carried off many horses and mules from the Durango area,[19] and in 1591 the enemy Indians were harassing the mines at Santa Barbara.[20] In 1593 Captain Francisco de Leyva y Bonilla set out from Nueva Vizcaya with a good sized party of soldiers and Indian auxiliaries to pursue the enemy Tobosos and Gavilanes of the Bolson de Mapimi region. The latter Indians, whose territories extended northward almost to Texas, had been making extensive raids, killing Spaniards and carrying off livestock.[21] Leyva de Bonilla's campaign failed to halt the Tobosos, however, and they were

16 Relacion . . . por Francisco de Ibarra.
17 Melchior de Alva, 1574.
18 Miranda, 1575, pp. 563-567.
19 Descripcion de la Villa de Nombre de Dios, 1608.
20 Río de Losa, 1591.
21 Villagutierre y Sotomayor, c. 1698.

to continue their warfare and raiding until the 1700-1750 period when they were finally liquidated by the Spanish. By 1604 the Tobosos' northern neighbors, the Chisos (living partly in Texas), were making direct raids upon the Spanish and they too were to continue their horse-acquiring activities until liquidated in the early 1700's.[22] The Concho Indians also became involved in warfare with the Europeans shortly after 1600.

Early in 1596 the Viceroy of New Spain reported to King Felipe II that the state of affairs in Nueva Vizcaya was the same as it had been in October, 1595, that is, that the province ". . . never lacks Indians who go about in rebellion and at war, and from whom the vecinos and settlers are not secure and they receive many damages to their persons and haciendas. . . ."[23] In 1597 a report noted that one priest went to visit unconverted Indians accompanied by some friendly Tepehuanes on horseback.[24] By 1608 it was asserted that none of the Indians in the Durango region (Tepehuanes) traveled on foot as all rode horseback. The ranches in the Nombre de Dios area were raising from 300 to 500 horses and mules each year.[25] Thus it is certain that by 1600 the Indians of the province of Nueva Vizcaya had large numbers of animals and it seems very likely that the hostile tribes to the north were regularly acquiring livestock on their raids. In 1604 Francisco de Urdiñola asserted that it was an ordinary experience on the frontier for a few Spaniards to establish a mining camp and then be wiped out by the hostile Indians who would kill the miners and carry off their mules and livestock. He pointed to what had happened at San Andres in 1601 as an example of this.[26]

There can be little doubt that horses and mules in small numbers at least were reaching the border areas of Texas from Nueva Vizcaya, Coahuila, and Nuevo León by 1600. Certainly there were many natives in the north who were familiar with the handling of livestock by that date, for many Indians from the *tierra adentro* were being enslaved and taken south to work as laborers in the Nueva Vizcaya settlements and in Saltillo and Monterrey. For example, the Indians of the La Junta region were raided periodically between c.1570 and 1600 and many of the La Juntans were able to subsequently escape and return to their people.[27] Unfortunately the diaries of the early expeditions to northern regions do not mention mounted (or un-mounted) Indians as being along the Rio Grande in the early 1580's (although the Rodríguez-Chamuscado expedition of 1581-1582 did leave a horse with a Caguate living on that river), but an account of 1590 does

22 Mateo de Vesga, 1624, pp. 142-143.
23 Luis de Velasco, 1596.
24 Annua de 1597.
25 Descripcion de la Villa de Nombre de Dios, 1608.
26 Urdiñola, 1604.
27 Forbes, 1959.

indicate that Indians of the lower Pecos River valley had obtained livestock of some kind. Gaspar Castaño de Sosa records that while marching up the Pecos River (near Girvin, Texas) a group of Jumano Indians (called "Tepelguan" and "Despesguan") carried off a number of oxen. Subsequently, in Apache territory (at Carlsbad, New Mexico) Castaño discovered ". . . a very large corral, where the Indians were in the habit of enclosing livestock. . . ."[28] That the natives of southern New Mexico should have had livestock in 1590 is not really surprising when one notes that Carlsbad is due north of La Junta.

On his return journey from New Mexico Castaño was bothered in the Las Cruces region by Indians who attempted to carry off some horses, thus indicating that the El Paso area natives were at least attempting to become mounted in the early 1590's.[29]

The advance of the Spanish frontier west of the Sierra Madre Occidental was much slower prior to 1600 than was the case in Nueva Vizcaya. Nevertheless, it is quite possible that some Indians had begun to acquire horses in Sonora by that date. From 1533 (the date of the successful Cahita resistance to Nuño de Guzman and the stabilization of the Spanish frontier at Culiacan) onward to 1610 the area of northern Sinaloa and southern Sonora was a land of war and slave raiding. Particularly in the early years the Spaniards of Culiacan were in the practice of capturing slaves as far north as the Yaqui region, and many of the slaves later escaped to their people. Likewise, many rebellions occurred and it seems probable that the northern Sinaloa groups acquired horses by this means prior to 1600.[30]

In spite of early opportunities the appearance of the mounted Indian in Sonora seems to have occurred relatively late, or, at least it is not possible at this time to document an early appearance. In the early 1540's the Spanish post of San Gerónimo de Corazones was destroyed by Sonorans and one might suppose that in this and other encounters with the forces of the Coronado expedition the Indians would have obtained livestock. It appears, however, that the Sonorans were not ready to receive the new trait of raising and caring for livestock. Thus they probably killed any animals which fell into their power. In 1565 Francisco de Ibarra led an expedition through Sonora to Casas Grandes, Chihuahua, and Baltasar de Obregón, chronicler of the journey, tells us that the Indians of the Yaqui River valley were ". . . astonished at the sight of horses, negroes [and other] things never seen by them."[31] In spite of this the natives of Guaraspi were able to steal horses from the Spanish, and by the time the Ibarra party reached Sahuaripa only 120 mounts remained out of an initial 300. Subsequently, worn out horses were

28 Castaño de Sosa, 1590, pp. 301-305.
29 Discurso de las Jornadas, 1598.
30 See Pérez de Ribas, 1645.
31 Obregón, 1584, p. 161.

abandoned and still others were eaten by the explorers while making their way from Chihuahua back to the coast.[32] Unfortunately, it is not possible to determine whether the Ópata and their neighbors became mounted in 1565. We have no knowledge of internal affairs in Sonora until the period of the 1620's to 1650's when the Jesuits began to missionize the Pimas and Ópatas. Subsequently Sonora does not appear to have greatly contributed to the spread of the horse into the United States until after 1682 when the Apaches, Sumas, Janos, Jocomes, Sobaipuris, Pimas, and Séris became involved in wars with the Spanish and raided the province for horses.[33] Even then the spread of the horse from Sonora was not uniformly rapid, for the Colorado River tribes were just beginning to acquire mounts in the period from 1744 to 1775.[34]

By way of summary then, it appears that the mounted Indian had developed throughout most of northern Mexico, with the possible exception of Sonora, by 1600; and furthermore that horses and other livestock were being traded as far north as the southern borders of Texas and New Mexico, this by a natural process of spreading out from central Mexico and not by means of "strays" left by any northern expeditions.

NEW MEXICO AND THE PLAINS: 1582-1680

A number of Spanish expeditions sought to discover wealth in the New Mexico region in the 1580's and 1590's. All of them took livestock into the north and there is, of course, a theoretical chance that the Indians managed to acquire some horses or mules. At least two specific opportunities need to be examined here. In June, 1581 the Rodríguez-Chamuscado party consisting of three friars, nine soldiers, and Indian servants made their way into New Mexico. When the group decided to leave the Tompiro pueblos and visit the plains Fray Juan de Santa María separated himself from the rest and attempted to return to Nueva Vizcaya. The Indians became alarmed, fearing that the priest went to get reinforcements, and after following the friar for two or three days they killed him, presumably acquiring (or killing) his livestock.[35] Shortly thereafter the Indians of the pueblo of Malagón managed to kill three horses belonging to the main Spanish party. The natives were punished when horsemeat was found in their possession.[36] In January, 1582 two of the friars decided to remain with all of the Indian servants at Puaray while the soldiers returned to New Spain. Subsequently, the priests were killed and their livestock must necessarily have fallen into Tiwa hands.[37]

32 *Idem*, pp. 174, 186, 193, 209.
33 Forbes, 1959.
34 Forbes, 1956, pp. 55, 58-60, 67.
35 Gallegos, 1581-1582, pp. 341-342.
36 *Idem*, pp. 342-343.
37 *Idem*, pp. 357, 359.

In 1593, as mentioned previously, Captain Francisco de Leyva y Bonilla led a squad of soldiers northward from Nueva Vizcaya to punish the Tobosos for their horse stealing. Once in the field, however, the Spaniards decided to seek wealth in New Mexico and a year was spent among the Pueblo Indians. Finding no wealth there the party traveled out onto the plains and was almost completely destroyed by the Indians of Quivira. Only a boy, a mulatto woman, and several Mexicans (Nahuans) escaped alive, the rest of the squad being killed in a fire set by the Indians. Theoretically, then, it is possible that the Quivirans acquired many horses from this party and utilized their young Spanish captive to teach them how to care for them. The evidence indicates, however, that all of the animals were killed in the fire or subsequently, for the Oñate expedition to the same area in 1601 found only horse bones and bits of Spanish equipment.[38] In later times the Quivira area does not appear to have been a horse-dispersal region (in 1682 the Pawnee were getting horses from the Plains Apache [Gattacka] and not from the Wichitans) and this factor argues against the survival of any horses. The same reasoning applies to a much earlier opportunity which occurred in 1542 when Fray Juan de Padilla's party visited the Quivirans and was nearly exterminated.[39]

When Juan de Oñate set out to conquer New Mexico late in 1597 he had some 350 horses, mares, colts, and mules, besides thousands of goats, sheep, oxen, and cattle. It is well to note that the number of mounts and pack animals was not great considering the fact that some 129 men and an undetermined number of dependents made up the expedition. Undoubtedly some of the men had their own riding horses but many of them were poor and Oñate had to donate 38 horses to individuals. This left the *adelantado* with only 81 horses, 101 mares, and 96 colts.[40] Early in 1598 Vicente de Zaldívar was sent northward to explore a route from the junction of the San Pedro and Conchos Rivers to El Paso (in order to avoid La Junta where the Indians were hostile). Zaldívar became lost and was forced to eat several of his horses and raid an Indian village in order to obtain guides. Interestingly, the guides were later to be rewarded with gifts of horses if they successfully led the Spaniards to El Paso, and this would indicate that the Indians south of El Paso had some access to mounts by 1598.[41]

Oñate and his army were eventually able to reach New Mexico and a Tewa pueblo, San Gabriel, was appropriated as headquarters by the Spaniards. Immedi-

38 Zarate Salmeron, 1626.

39 Castañeda, 1562, p. 263.

In 1719 the French trader Du Tisné visited the Osage and the Panis and from the latter he purchased two horses and a mule with a Spanish brand. The Panis were apparently getting their livestock from other areas (Nasatir, 1952, p. 19).

40 See Hammond, 1927, pp. 14, 215-228.

41 Villagutierre y Sotomayor, 1698.

ately the governor launched a series of exploratory (and confiscatory) journeys and on one of these, between Ácoma and Zuñi, some horses were lost late in October, 1598 and never recovered.[42] On December 4, 1598 Juan de Zaldívar and seventeen men entered Ácoma to confiscate supplies, leaving other men to guard their horses. The Keres killed fifteen of the Spanish group and apparently had an opportunity to acquire some mounts, but whether they did or not is not too important since their pueblo was destroyed by Vicente de Zaldívar early in 1599.[43] In 1601 several Spanish soldiers attempted to desert New Mexico but they were attacked by the Tompiros and twenty of their horses were killed. The Tompiro pueblos involved suffered an Ácoma-like fate.[44]

The Spaniards who came to New Mexico were used to having Indians perform such tasks as involved manual labor and very soon the unfortunate Tewas of San Gabriel were put to work herding and guarding livestock. By 1600 the Pueblo Indians were performing such service, probably in company with Indian slaves and servants brought north from Nueva Vizcaya. This is, of course, a very significant development, especially when it is noted that during the entire Oñate period (1600-1610) the Pueblo Indians were fleeing in large numbers to the mountains and to the Apaches.[45] It should also be noted that all during the period from 1600 until at least the 1630's the Apaches of the plains were accustomed to come into central New Mexico each year to trade with the Pueblo Indians. They brought their whole villages and camped near the pueblos with which they were going to trade and thus had excellent opportunities for observing the Spanish and their horse riding techniques.[46]

In December, 1600 supplies and livestock were sent to New Mexico by the viceroy along with an inspector to check up on Oñate. It was discovered that the Spaniards had been very wasteful and had eaten almost all of their cattle and oxen and most of their sheep. When the viceroy's men returned to Mexico they reported that the new province had from 150 to 160 fighting men, 500 to 600 horses, and 150 to 300 mules. One of the declarants stated that Oñate's party had originally had 1500 horses but that they had lost many, others had died, and still others had been found killed by arrows. Thus with those brought north in 1600 they had only 500 left.[47] It appears possible, then, that the refugee Pueblo Indians and

42 Bolton, 1916, pp. 234-235.
43 Hammond, 1953, vol. 1, pp. 326-427.
44 *Idem*, vol. 1, pp. 615, 789, 805.
45 Vrondate, 1601; Espinosa, 1601; Ortega, 1601; see below in our text.
46 Vrondate, 1601; Leandro, 1601; Ortega, 1601; Forbes, 1959.
47 Leandro, 1601; Ortega, 1601.

their Apache friends had horses by 1600. On the other hand, several men sent by Oñate to Mexico declared that the province had 200 men, 200 to 500 mares, 800 horses, and 300 to 700 mules.[48]

In June, 1601 Oñate set out on a journey to discover wealth in Quivira. His party consisted of some 70 men and 700 horses and many of the latter appear to have been lost on the plains.[49] When he led his army back to San Gabriel he found that most of the other Spaniards had deserted New Mexico, probably taking considerable livestock with them. This desertion had the further significance that for the next few years there were few soldiers to keep track of the livestock which remained and the Indians may have acquired more animals. When Oñate returned from another expedition in 1605 there were only 80 men at San Gabriel to greet him.[50] Undoubtedly, the numbers of horses and mules possessed by the Spaniards were not very great at this time. Thus the numbers possessed by the surrounding Indians could not have been very large either, and there is the additional factor that the Pueblo Indians were in a piteous condition in 1600-1610 due to the ravaging of their pueblos by the Spaniards. It is quite possible that the Indians ate any horses and mules which they acquired.[51]

A new era for New Mexico was ushered in with the establishment of an encomienda at Jemez in 1606. During the early period of Spanish activity in the province the Jemez were quite warlike, anti-Spanish, and close allies of the Navaho (the latter are called "Jemez Apaches" by the Tewa), and thus it is not surprising to find the Athapaskans soon involved in warfare with the encomendero of Jemez, Juan Martínez de Montoya. The Navaho and other Apaches were in a close alliance with the still-free Pueblo Indians and with various refugee groups and thus once the fuse was lit the Spaniards were in serious trouble. Prior to 1606 there had been no warfare in New Mexico except for the Ácoma affair, the Tompiro troubles, and a campaign to Taos in 1602, but from 1606 onward to at least 1614 there were to be frequent encounters with the Navaho, the Jemez, and their Pueblo and Apache allies. Late in 1607 or early in 1608 Fray Lazaro Ximénez went to Mexico to report on affairs in New Mexico and he declared that the Spaniards were ordinarily molested by the Apache Indians who were burning and destroying pueblos, killing people, and ". . . carrying off the horse-herds. . . ."[52] Warfare continued throughout 1607, 1608, and 1609. In the last year the viceroy asserted that the Apaches

48 Vrondate, 1601; Espinosa, 1601.
49 Hammond, 1953, vol. 2, p. 797.
50 Hammond, 1927, p. 31.
51 Forbes, 1959.
52 *Ibid.*

are usually a refuge and shelter for our enemies, and there they hold meetings and consultations, hatch their plots against the whole land, and set out to plunder and make war.

The new governor, Pedro de Peralta, was ordered to congregate the dispersed Pueblo Indians in settlements in order to keep them from mingling with the Apache.[53] Thus it is clear that by 1606-1609 the Athapaskans of New Mexico were acquiring herds of horses and that they were coöperating with refugee Pueblo Indians who probably had experience with handling livestock.

It is not necessary to review every individual case of horse acquisition by the Indians of New Mexico from 1610 to 1680, nor is it possible to document the arrival of the horse among each and every Athapaskan or Puebloan group. The Apaches to the north, east, south, and southwest of New Mexico remained largely at peace with the Spanish until c. 1629 and c. 1639 when treacherous slave raids goaded the Plains Apache into hostilities for brief periods. Under the rule of Luis de Rosas, 1637-1641, ruthless tactics goaded many Athapaskan groups into warfare. The Navaho carried off many herds of horses; the Gila Apaches became hostile (apparently for the first time); the Sumas took up the song of war; and Taos and Jemez revolted. Furthermore, the Spaniards fought among themselves and the province was in a state of anarchy, with the Indians getting large numbers of animals with ease. Even more significant was the fact that the Indians of Taos abandoned their pueblo in c. 1639 and fled to the Plains Apache in western Kansas where they established El Cuartelejo. Prior to 1642 many of the Taos people were forcibly brought back to New Mexico but a small party remained in Kansas until 1662. During their period of exile the Puebloans must have exercised some considerable influence upon the Apache, including introducing the latter to the use of the horse; that is, if the Athapaskans did not already ride horses.[54] Thus the arrival of the horse in western Kansas can be dated at least as early as c. 1640.

Luis de Rosas' many crimes included making an attack upon the peaceful Utacas (Utes), and many prisoners were forced to work in a factory which the governor set up in Santa Fé. Undoubtedly many of the Utes were able to escape to their people after the death of Rosas (1641) and they very likely carried hatred of the Spanish and a knowledge of horse-using with them.[55] This, of course, is of great significance since the Utes were later to be important in trading horses to more northerly tribes.

In 1650 the Tiwa, Keres, and other Pueblo tribes allied themselves with the Apache in order to throw off the Spanish yoke. As a part of a prearranged strategy

53 *Ibid.*
54 *Ibid.*
55 Salazar, 1641.

the Puebloans turned over several horse herds to the Athapaskans but the Spaniards were able to act in time to prevent the success of the plan. In the late 1650's under the harsh rule of Bernardo López de Mendízabal the Apaches were goaded by slave raids into increased hostility; large numbers of animals were carried off, especially by the Navaho. By the 1660's the Spaniards were no longer worried about keeping horses from the Indians for they were trading mounts for Quivira slaves with the Plains Apache at Pecos.[56]

Apache-Spanish relations had been steadily worsening in the 1640's and 1650's and by 1667 most Athapaskan groups were ready to declare war to the end. In that year a series of revolts among the Piros, Tompiros, Mansos, and Nueva Vizcaya tribes occurred and the Apaches of Gila and Siete Ríos (southeastern New Mexico) were recruited as willing allies. The Puebloans were again to turn horses over to the Athapaskans but, as before, the Spanish proved too strong. Nevertheless the Apaches continued their warfare, which by the 1670's had become relentless; herds of horses, mules, cattle, and sheep were carried off. By 1679 the province was so depleted of livestock that more horses had to be brought northward from Nueva Vizcaya! Thus the years 1667-1680 can probably be considered as that period in which really large numbers of horses were dispersed from New Mexico.[57] The extent of this dispersal can be seen in the fact that when La Salle was at Fort Crevecoeur in 1682 he met an Oto Indian who had a horse's hoof and who declared that the "Spaniards" were making cruel war upon the Otos. These "Spaniards" were undoubtedly Plains Apaches (Padoucas) for we know that in the early 1700's the latter were at war with the Otos.[58] La Salle also met a party of Missouris who had a Pawnee among them. The latter people were said to be allies and neighbors of the Gattacka and Manrhoat, who lived to the south of the Pawnee and from whom the latter were obtaining horses. The Gattacka and Manrhoat were thought to be robbing the horses from the Spanish of New Mexico, and in part at least this was true, since Gattacka was the Pawnee name for the Apache (applied commonly to the Kiowa Apache in later times).[59]

The revolt of the Pueblo Indians and their Apache allies in 1680 has commonly been regarded as that event which really contributed to the dispersal of the horse. There is no doubt that the rebels acquired large herds of animals, as in the Isleta region where the Apaches were said to have had the guns and horses of 120 men they had killed besides an even larger number of mares and cattle.[60] During the actual rebellion mounted squads of Pueblo Indians went about gather-

56 Posadas, 1688.
57 Forbes, 1959.
58 Hennepin, 1682, p. 627; Nasatir, 1952, p. 17.
59 Margry, 1877, vol. 2, pp. 201-202.
60 García, 1680, p. 69.

ing up the cattle and other property of the Spanish and for eighteen years the mounted Puebloan was to be a serious obstacle to any reassertion of Hispanic rule.[61] Nevertheless, it is well to remember that the Apache had been carrying off the province's horses for many years and that the Spanish took large numbers with them when they retreated to El Paso. By 1682 the Plains Apache were forced to go to the Gila country in order to acquire the horses they had previously purchased from the Spanish at Pecos. The Gila Apaches had apparently confined their raiding to the Rio Grande valley until about the year 1682 when they attacked ranches in the vicinity of Casas Grandes, Chihuahua. Subsequently the Spanish discovered that

> among the apaches that came to that ambush there came a great number of apaches of the plains (these are from towards the east and their country is distant 200 leagues from where the ambush took place). These [Apaches of the plains] used to enter into New Mexico to barter and trade because they always were at peace, but this relationship having ceased [since 1680] they come now to have it with the Apaches of the Sierra de Jila, who rob them in Sonora [and] slip away with horses which they [the Apaches of the plains] very much crave, as much for the war against other infidels, their enemies, in whom they make great havoc, as for the chase of the buffalo which is their sustenence. . . .[62]

If the Plains Apache were forced to come all the way to the frontier of Sonora to acquire needed horses in 1682 it means that the New Mexico region must not have had a surplus of horses, in spite of the Pueblo Revolt.

To summarize, it is possible that the Pueblo Indians acquired some horses as early at 1582 but the first definite record of horse acquisition is in 1606-1607. From 1606 until the late 1620's the Navaho and the Indians of Jemez were the principal opponents of the Spanish and therefore the principal raiders for livestock. From 1639 onward hostilities between European and Indian increased steadily in New Mexico until a peak was reached in the 1667-1680 period. The Spaniards apparently lost the greatest numbers of horses in the years of 1607-1610, 1620's, 1639-1642, 1650, late 1650's, and from 1667 to 1680.

As far as Arizona is concerned little can be said of the period before 1680. The Hopi acquired livestock after the 1630's as a part of the process of missionization and they made the animals truly their own with the revolt of 1680. The Apaches probably acquired mounts gradually after 1630-1640 and the Havasupai may have gotten some horses when they were partially missionized in the 1660's

61 Otermín, 1680, p. 8.

62 Otermín, 1682.

and 1670's. Little is known of the Pimans until 1688 when the Sobaipuris were involved in a war with the Spanish of Sonora.[63]

NORTHERN MEXICO: 1608-1680

The Indians of Nueva Vizcaya, Coahuila, and Nuevo León continued to acquire large numbers of horses during this period: the numbers involved appear to have been much larger than those in New Mexico (primarily because there were hundreds of haciendas in the region from Santa Barbara to Monterrey). Indian wars in Chihuahua were almost continuous but only the most significant ones will be mentioned here. From 1616 until 1621 the region was devastated by a revolt of the Tepehuanes allied with Tobosos, Chisos, Conchos, Salineros, and some Taraumares. The rebels destroyed many settlements and carried off herds of horses and mules. The mining towns of Guanaceví and Indehé were destroyed and all of the livestock haciendas and farms were sacked by the Indians. The cost of the affair to the crown was said to be 700,000 pesos.[64]

By the 1620's there were herds of wild horses in Chihuahua. In 1628 a Spanish party lost fifteen mules which ". . . ran away after the herds of wild mares. . . ."[65] In 1644 another important revolt occurred when the Tobosos rose, sacking a ranch near Santa Barbara and carrying off all of the cattle and several horse herds. Subsequently they carried off mule herds from Parral, 1500 animals from Indehé, and 2000 head of cattle from near the latter place. The Tobosos took these animals almost to the Rio Grande, to which place they were pursued by the Spanish. Late in 1644 or early in 1645 the Julimes (of La Junta), the Conchos, the Colorados (of northern Coahuila), and many other bands joined the Tobosos in stealing a ". . . very great quantity of horses and livestock. . . ."[66] The same type of raiding continued for many years. In 1648 the Taraumares and some Tepehuanes joined the rebellion and a great number of cows, mules, and horses were carried off by them. Between 1648 and 1651 the Taraumares ran off all the livestock in the Satibo region while Tobosos and Salineros acquired herds of mares from the valley of the Río Florido.[67] In 1652 the Laguneros of San Lorenzo revolted and robbed large numbers of animals from ranches in the Parras region.[68] In the last two months of 1653 the Vizcaya rebels carried off 400 horses from the area of Parral, not to mention raids on other areas.[69] These instances should be

63 Forbes, 1959.

64 Valdes, 1647; Relacion de los sucesos que tubo la guerra de los Tepehuanes, 1618; Valencia, 1618; Andres Pérez, 1638.

65 Perea, 1632, p. 210.

66 Zepeda, 1645, pp. 130-135; Alegre, 1768, pp. 244-245.

67 Pasqual, 1651; Valdes, 1648; Nueva Montano, 1648.

68 Contreras, 1653.

69 Apressa Falcon, 1654.

sufficient to show that the Indians of Nueva Vizcaya were acquiring great numbers of horses throughout the 1600's.

Several interesting points remain to be noticed before proceeding to a discussion of Coahuila-León affairs, namely that by 1678 the Taraumares of San Felipe were raising ". . . oxen, and some small livestock, and almost all were raising horses . . .";[70] and that in 1681 it was said that the hostile tribesmen of La Vizcaya were ". . . expert and so able in the management of arms and horses . . ." as to pose a serious threat to the Spanish.[71] The menace of the Indian became even greater in 1684 when the Sumas, Janos, Mansos, Conchos, Julimes, and many other tribes joined the Tobosos as enemies of the Spanish. In the next two years over 3,000 head of small stock, and more than 1,000 horses and mules were carried off from Casas Grandes while more than 6,000 animals were taken from Parral.[72] However large these figures may seem, they amount to little as compared with the 66,155 head of livestock (cattle, mules, and horses) carried off from Nueva Vizcaya by the Apache between 1771 and 1776.[73] The latter figures are inserted to show that La Vizcaya continued to serve as a source of horses after 1700.

In 1621 it was said that the road from Zacatecas to Saltillo was frequented by innumerable herds of wild mares.[74] With this introduction it should be clear that the Indians of Coahuila had no trouble in getting horses. The Indians of Nuevo León perhaps found it more difficult, for it is recorded that throughout the 1650's and 1660's they were raiding for horses, mares, and other livestock and retaliating for Spanish slave raids.[75] Tribes along the border of Texas were frequently involved in these wars, but more of this will be mentioned below.

THE TEXAS AREA: 1600-1690

As noted previously it appears that horses began to reach the La Junta area as early as 1574 and the lower Rio Grande region by 1590. It appears certain that throughout the 1600's livestock continued to spread northward into Texas and by 1680 (at the very latest) all of the tribes of that area had horses in relative abundance. Throughout the period under consideration the La Junta region served as a gateway for the transmission of traits from New Spain to Texas and the vehicle for this movement was the Jumano Apache.[76] The Jumanos traded with the Julimes at La Junta and then carried goods to the tribes of east Texas. Unfortunately, it is not possible to say with certainty when the former acquired horses

70 Relacion de las misiones que la Compania de Jesus tiene en Nueva Vizcaya, 1678.
71 Solís Miranda, 1681, p. 232.
72 Nicolas de López, c. 1686.
73 Resumen General . . . , 1776.
74 Arregui, 1621, p. 129.
75 De León, n.d., pp. 218-220.
76 See Forbes, 1959; Kelley, 1955.

but one can assume that it was only shortly after their acquisition by the Julimes. As mentioned earlier the latter people were at war with the Spanish from c. 1570 to 1600 and then they were allied with the Tobosos in the revolt of 1644-1645. Subsequently, in 1677 they were said to be robbing horses in company with the Chisos and Tobosos.[77] The Julimes and the other tribes of the Río Conchos did not have to increase their horse herds by raiding, however, for we know that by the 1680's (and probably much earlier) they were working seasonally in the Parral region where they could easily purchase horses.[78]

The Jumanos were usually friendly towards the Spanish (trading with parties from New Mexico, for example) but one of their bands, the Sibolos, was making raids in the Parras region in 1673, being allied with the Colorados, Cacastles, and other northern groups. In 1677 they were among bands raiding along the Río Nazas.[79] Both Parras and the Río Nazas were hundreds of miles from Sibolo territory and thus we can be sure that they were mounted by the 1670's. The Chisos of the Big Bend country were raiding the Spanish of La Vizcaya as early as c. 1604, their raids continuing until they were missionized and destroyed in the early 1700's. There can be little doubt that they became horse-using Indians quite early, for the distances they traveled in their raids were very great.

The spread of the horse into east Texas was a gradual process extending from the 1590's onward. The tribes to the north of Nuevo León were active raiders and in the 1650's the Cacaxtles (mentioned above as Cacastles) were particularly active in carrying horses and livestock from the Monterrey-Cerralvo region. As a result, the Spaniards made a campaign seventy leagues north of Monterrey in 1653, acquiring 125 slaves. In 1655 another *entrada* was made against the Cacaxtles twenty-four leagues north of the Rio Grande.[80] Thus it is positively known that east Texas Indians had horses by the 1650's. In the 1660's the "Nations of the North" continued to steal horses in Nuevo León. 1667 and 1668 were particularly bad years for the Spanish as the Borrados of southeastern Texas, the Pelones of the Sierra of Papagayos, and the Tetecuaras of northern León and Coahuila carried warfare to within a league of Monterrey.[81]

There can be no doubt that the Indians of northern Coahuila and Nuevo León had trading contacts with Texas tribes farther to the north, especially with the Hasinai Caddo (the Texas). In the 1670's missions were established in northern Coahuila and in 1676 the Bishop of Guadalajara said of the province after a visit,

77 Testimony of the Alcalde of Cuencame, 1677.
78 Posadas, 1688.
79 Forbes, 1959.
80 Bolton, 1912, p. 14; De León, n.d., pp. 221-222.
81 De León, n.d., pp. 243-244.

Coahuila . . . has a neighbor on the north, inclining somewhat to the east . . . which they call Texas . . . they have crops, wear clothes, and punish misdemeanors. . . . The Coahuiles do not give more detailed reports of the Texas because, they say, they are allowed to go only to the first pueblos of the border. . . . There are many of these Coahuiles who give these reports, and who say that they got them through having aided the Texas in their wars against the Pauit, another very warlike nation.[82]

Thus one would expect that the Hasinai and other northeast Texas tribes would have been getting horses from Coahuila-Nuevo León at least as early as the 1670's. At the same time they were probably getting horses from the Jumanos.

In 1683 a group of Jumano Apaches under Juan Sabeata visited El Paso with the object of enticing the Spaniards into a trading and fighting venture. Among other things Sabeata declared that the Texas (Hasinai) were feeding their horses with maize.[83] Shortly thereafter a Spanish party journeyed with Sabeata into central Texas (on the Colorado River near the junction of the Conchos). The diary of the expedition reveals that the Jumanos had horses (which their Apache neighbors were trying to steal) and also that Coahuila Indians (who were missionized) were traveling as far as the latter region.[84] Our most enlightening information on the extend of horse-use in Texas in the 1680's is provided, however, by the accounts of the La Salle expedition. Leaving his newly established fort on the Texas Gulf coast (Fort St Louis at Matagorda Bay) in April, 1686 La Salle traveled for three days to the northeast where he met a party of Indians, some of whom were on horseback. These Indians knew about the Spanish. Farther to the north and east the French met the Rirononas who described the Spanish as bad and cruel and wanted La Salle to aid them in an attack upon the latter. Still farther to the east the French reached the Cenis (Hasinai) where they were immediately able to purchase horses. They found among the Cenis

many things which came undoubtedly from the Spanish, such as pesos fuertes and other money, silver spoons, silk goods, gold or silver, clothing and horses and among other things we saw a bull of Rome or of the Holy Crusade. . . . The horses are common here, and they exchange them for a hatchet. . . .

It was further found that the Cenis

have relations with the Spanish by means of the Choumanes [Jumanos] allies of the Cenis and that they are always at war with New Spain. . . . They count it as six days journey to the Spanish, of whom they gave a description so natural that no doubts

82 Bolton, 1912, p. 16.

83 Sabeata, 1683.

84 Bolton, 1916, pp. 330-332, 338.

remained to us, although the Spanish haven't as yet begun to come to their village, and only their warriors have united themselves with the Choumanes in order to make war on New Mexico.[85]

La Salle was able to meet a Jumano ambassador among the Hasinai. Thus it is clear that the Texas were acquiring their horses from New Spain by way of the Jumano-La Junta route at this time.

It appears that all of the east Texas tribes had horses by the 1680's. In 1687 it was mentioned that the Indians around Fort St Louis grew maize and had horses. Farther inland the Quoanoatinos (probably a Tonkawan group) sold horses to the French at low prices, and it was said that they made war upon the Spanish. Still farther to the north were the Tharas, Tiakappanes, and Paloussas, all of whom had horses. In summarizing the state of affairs in Texas Fray Anastasio Donay asserted that horses were so common there that the Indians felt well paid with the presentation of one hatchet for a horse; and that in an area of 150 to 200 leagues there were few nations who lacked animosity towards the Spanish.[86]

Spanish expeditions into east Texas in the early 1690's confirm the statements of the French in regards to the presence of horses among the Indians. In 1691 the Terán party met a group of Jumanos, Sibolos, Cholomes, Catquesas, and others near San Antonio who had horses. The Spanish noticed that the latter had some very small saddles with reins and they were told that the equipment had been taken in war from the Apaches.[87] The latter were said not to be very valiant because they fought with "armed horses." Later in 1691 Fray Francisco de Jesus María of the newly founded mission among the Hasinai was describing the tribes who were enemies of his charges, among them were the Sadammo. This nation was

very large, all their houses are covered with buffalo skins, they have many horse herds, mule herds, clothing, and many instruments of iron. . . . They are not striped and they say that the larger part of them are dressed. They are enemies of all the nations friendly to the Asenays.[88]

Horses continued to be abundant in east Texas in the 1700's. In 1716 it was said that each of the Hasinai chiefs had four or five horses and that some of the rest of the people had two and three apiece.[89]

From the evidence presented it should be clear that the dispersal of the horse throughout the Southwest was not exclusively or even primarily a result of the

85 Donay, 1686.
86 *Ibid.*
87 Massanet, 1691.
88 Jesus María, 1691.
89 De Velasco, 1716.

colonization of New Mexico and the Pueblo Revolt of 1680. It seems clear that the bulk of the horses in the eastern half of Texas at least were the result of trading and warfare on the Nueva Vizcaya, Coahuila, and Nuevo León frontiers, and it has already been pointed out that the 1680 revolt may not have created a great surplus of horses in New Mexico.

The appearance of the mounted Indian in northern Mexico began in the 1550's; by 1600 most of the Indians of that area (with the exception of Sonora) probably had horses. The spread of riding animals into the southwestern United States apparently began in 1574 at La Junta and in 1590 farther to the east. After 1600 the northward movement of the horse continued, being greatly facilitated by the actual transporting of animals into New Mexico by the Spanish. By 1680, the terminal date of this study, the Indians of all of Texas and New Mexico were mounted; many Arizona groups had livestock; and horses were being spread to even more northerly regions by the Plains Apache and the Utes. In 1682 the Pawnee were said to be getting horses from the south.

It is interesting to note, by way of a conclusion, that the spread of the horse northward into the Southwest was primarily an Indian phenomena. Wherever possible the Spaniard attempted to interfere with the process: it was only when the Indian already had access to mounts that active horse trading was carried on by the Spanish. Thus the importance of inter-Indian relations in American history can readily be seen.

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Abbreviations:

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B.M.H.	Biblioteca del Ministerio de Hacienda, Madrid, Spain.
B.N.	Biblioteca Nacional, Madrid, Spain.
M.N.	Museo Naval, Madrid, Spain.

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LEADERSHIP AND STATUS CONFLICT ON ROMÓNUM, TRUK[1]

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IN THIS PAPER the failure of the leaders of kinship groups on Romónum island (Truk atoll, Caroline Islands) to exercise the power inherent in their positions will be examined and an explanation for this phenomenon will be sought in terms of the social position these leaders occupy.

The lineage, or *famani,* is the basic kinship group on Romónum and each has a chief who is almost always the oldest non-senile man in the group. In addition, there is also a leader of each adult generation within the lineage who attains his status by being the oldest in his sibling group. Since there are generally two adult generations in the lineages on Romónum, what is commonly found is the senior man of the parental generation in the status of lineage chief and the eldest male of the younger adult generation as the leader of his own generational group. Informants were evenly divided as to whether or not the senior man of the younger generation would be lineage chief if he were older than the senior man of the parental generation. Whatever the outcome if such an unusual situation should arise, the informants report that normally the senior man of the younger generation is subordinate to the lineage chief in all matters even if they concern property held by the younger generation apart from the lineage as a whole. The concensus was that the leader of the younger generation could do nothing of which the lineage chief did not approve. Despite these reports of his subordination, observation revealed that the younger leader asserted himself rather freely while the lineage chief was extremely reluctant to exercise his authority either over the lineage as a whole or over members of the lineage as individuals. This held true with respect to matters ranging from quarrels between spouses to the use of lineage property. Furthermore, observation indicated that the lineage chief was as reluctant to exercise his authority indirectly as he was to use it more openly.

An insight into the basis for the lineage heads' failure to exercise much active leadership was obtained from a series of formal, highly structured interviews concerning behavior toward kinsmen which were held with a carefully selected sample of informants. Although the total number of informants who participated in these interviews was larger, for the purposes of this paper the results of the interviews

1 This paper, in a slightly different form, was read at the 1958 meeting of the American Anthropological Association. I am indebted to Arnold Levine, Jerry Meyers, and Cecil French for valuable comments and suggestions. Homans' hypothesis, discussed below, was called to my attention by Dr French.

with eighteen adult individuals are most important. The information summarized in the table was gathered from a group made up entirely of men, none of whom has had extensive contact with non-Trukese culture. The total group is divided into six men who are over 40 years of age and twelve who are under 40. Without exception the men over 40 belong to the oldest generation in their respective lineages while none of the younger men do. Five of the six men who are over 40 are chiefs of their lineages and eight of the twelve men under 40 are the leaders of their generation groups.

Since the nature of the interviews is discussed in detail elsewhere[2] a very brief description will suffice here. Each informant was interviewed two or more times, with one session being devoted to inquiring about what behavior he felt he was able to express to his relatives in the abstract and another given over to asking what behavior he had actually expressed to specific kinsmen. The questions were designed to give each informant separate but comparable ratings on the amount of behavior he thought he could express and on the amount he had actually expressed. The purpose of these interviews was to supplement the data derived from participant observation with information which was not easily obtained in this way. Specifically, the aim was to find out what informants thought themselves able to do with respect to their relatives and what they said they had actually done. This last could be verified to some extent by observation of interaction among kinsmen and would be in a form which would allow comparison with what the informants reported themselves able to do. The questions in the interview were grouped together under a number of different headings, three of which concern us here. These three are "dominance," "submission," and "aggression." The heading "dominance" included three questions about the informant's ability to perform and three *separate* questions calling for his reported actual performance of the following behavior: tell kinsman to do work for informant; tell kinsmen to be less lazy; and tell kinsman to go rest. If the informant said he believed himself able to do all three with respect to a given kind of relative, his rating for that kind of relative would be three points on what I call the potentiality scale. If the informant said he had actually carried out all three of the actions with respect to some specific kinsman, his rating on what I call the descriptive scale for that kinsman would also be three. A negative answer on either scale reduces the informant's rating on that scale by one point. All the questions on the potentiality scale were asked in one session, while all the questions in the descriptive scale were asked in a separate ses-

2 M. J. Swartz, *The Social Organization of Behavior* (Ph.D. thesis, Harvard University, Cambridge, Massachusetts, 1958); *Idem, Kinship and Behavior on Romónum, Truk* (Manuscript in preparation).

sion held some days or weeks after the first. "Submission" included two questions (and thus a maximum rating of two points) concerning relatives dominating the informant, and "aggression" had two questions concerning the informants overtly manifesting anger to their relatives. The figures in the table are the result of taking the over-all mean of the mean scores for all relatives of informants in each group.

A glance at Table 1 reveals a consistent pattern in which the mean ratings of

TABLE 1

Mean Ratings and Standard Deviations for Male Informants of Two Age Groups on the Potential and Actual Expression of Three Kinds of Behavior

Behavior	*Potentiality Scale*		*Descriptive Scale*	
	Over 40	*Under 40*	*Over 40*	*Under 40*
Dominance	2.62	2.52	1.88	2.43
s.d.	.57	.75	.92	.81
Aggression	1.72	1.58	1.00	1.21
s.d.	.70	.74	.84	.73
Submission	1.71	1.48	1.10	1.45
s.d.	.32	.43	.85	.74

the older group are uniformly higher on the potentiality scale but lower on the descriptive scale for all three kinds of behavior. This means that the older informants consistently say that they *can* do more than the younger informants say they can, but despite this it is the younger informants who say they have actually expressed more of all three kinds of behavior. Comparing the means of the two groups with the *t*-test reveals that the differences are not great enough to reach customarily accepted levels of significance. However, since the direction of the means is so uniform, the aim here is to use the pattern indicated by these statistics in order to suggest an explanation for the lineage chiefs' unwillingness to exercise their power.

Taking the results for dominance and aggression first, we can easily infer that the older men do believe themselves to have considerable power to direct their kinsmen's activities and to enforce that direction. Observation revealed that the older men's statement that they could dominate and aggress a good deal if they

wished to was not idle boasting. When a non-senile older man chose to exert his authority directly, few if any of his lineage mates were willing to oppose him, especially if he were the lineage chief. This indicates that the problem here is not simply a conflict between the real culture and the ideal culture as concerns the status of the older men. They really do have the power to direct the activities of their kinsmen if they wish to use it, but as the descriptive scales indicate, they do not often do so. The problem, then, is to understand why the older men choose to exercise their power so little.

The ratings for submission provide a key for the solution to this problem. These ratings show that the older men report that their relatives can order them about more than the younger men say theirs can, but it is the younger men who report actually submitting more. This might appear contrary to the dominance-aggression results, but actually the data for all three kinds of behavior taken together provide a quite consistent pattern revealing the conformity of the older men to the value placed by Romónum people on humble and unassertive behavior. The older men, as leaders, report that they *could* dominate and aggress a great deal, but in accord with the value on unassertiveness, they do not do so. The older men, in accord with the valued humility, express considerable willingness to submit to their relatives, but these relatives do not exploit this willingness very much because of their acceptance of the older men's high status.

Although the results of the older men's reports are explainable in terms of the two values mentioned, the difference between their ratings and those of the younger men cannot be explained in this way since the values in question apply to everyone. The discrepancy between the two groups can be understood, however, if it is recognized that the older men must conform to the valued behavior more closely because the advancing years which have brought these men to the status of lineage leader are also bringing them toward the status of senile dependent. Men who are too old to perform the strenuous work of food production—none of the informants here had reached that point—are dependent upon the bounty of their kinsmen for the necessities of life and if their kinsmen do not choose to care for them sufficiently, they will suffer and even starve to death. Concern about this dependence is evident among all men, but it is naturally most acute among the older men who are beginning to be faced by declining physical strength. It is because of their realization of approaching dependence upon the goodwill of their kinsmen that the older informants conform so carefully to the values of humility and unassertiveness. The status they occupy as lineage chiefs or future lineage chiefs provides them with a legitimate basis for assertiveness, but their quickly approaching status of senile dependent makes it essential to their well-being that

they ingratiate themselves with their kinsmen by adhering to the generally valued non-assertive behavior. As one lineage head pointed out to me, it is particularly meritorious for persons in positions of power to be humble and passive since this shows they have no wish to put themselves above people—a cardinal sin on Romónum.

The failure of the older men to exercise the power inherent in their status as lineage leaders is to be seen, then, as due to the requirements of another status that these same men will soon occupy. In order to gain the affection of their kinsmen necessary to their well-being in the near future, they must antagonize these kinsmen as little as possible in their behavior as leaders. Since the kinds of behavior associated with active leadership run counter to behavior which is highly valued, this means that the old men will be reluctant to assert their authority. In short, performance of the role associated with the status of lineage chief would endanger their well-being in the status of senile dependent.

As the table shows, although the younger men do not believe themselves to have as much power to dominate and aggress as have the older men, it is the younger men who report actually expressing more dominance and aggression, and observation bears out their reports. The greater assertion on the part of the younger men is largely due to the absence, for them, of the conflict in statuses which is present for the older men. The younger men are more willing to expose themselves to the possible ill-will of their lineage-mates because they are not faced with imminent dependence on these relatives. However, it will be remembered that the authority of these younger men extends only to their peers and has no wider application. One interpretation of these findings is that on Romónum, and probably the other islands in Truk atoll as well, we find a culture in which a real power for leadership is present, but that this power is rendered largely ineffective by the values contrary to its exercise. This is accomplished through having the power in the hands of individuals against whose interest it would be to exercise it freely and by denying wider power to individuals who might be in a position to use it. This interpretation supports, in part, a hypothesis advanced by Homans.[3] This is that "the higher the rank of a person within a group the more nearly his activities conform to the norms of that group." As has been shown, the leaders of groups on Romónum conform quite closely to the value placed on unassertiveness and this conformity is made almost essential for them by their forthcoming dependence on the lower ranked members of their lineages. In the light of Homans' hypothesis, then, the status conflict can be seen as a social control which preserves a major value from violation by those who would otherwise be in a position to carry out such violation.

3 G. Homans, *The Human Group* (Harcourt, Brace: New York, 1950), p. 141.

However, a complication arises when it is seen that the preservation of the value on unassertiveness necessitates a violation of another value held by the group. This is that leaders should be wise *(tipachem)* and diligent in directing the activities of their group. One often hears individuals expressing discontent with their lineage leaders for failing to take a more active part in decision-making and, especially, in the settling of quarrels involving lineage members. Clearly the lineage chief is in a poor position to enter into such activities freely, since to do so would be inconsistent with his desire to ingratiate himself with his followers. Any decision the chief makes not only stands as a possible irritant to someone but also detracts from his standing as a humble man since it implies he believes himself to have superior wisdom.

It can be seen, then, that it is not enough simply to point out that the leader of a group will conform closely to the norms of that group. In this case the group has conflicting norms and it is only through a recognition of the entire social position in which the lineage leader finds himself that it is possible to appreciate why the leader acts in accordance with the valued humility and unassertiveness even when doing so causes him to come into conflict with another value held by his group.

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